JOURNAL
OF THE
WASHINGTON ACADEMY OF SCIENCES

VOL. 46 MARCH 1956 NO. 3

PHYSICS.—*The basis for standards for radiation protection.*[1] LAURISTON S. TAYLOR, National Bureau of Standards.

In the broad sense, radiation protection is a problem that will touch the lives of most of us more and more as time goes on, whether it be in connection with nondestructive testing, with radiology, or with the production of nuclear power. The uses of radiation-producing machines and the use of radioactive materials will undoubtedly increase as time goes on, and radiation as such will become an increasingly important part of our national economy. It will also become a part of our political economy because, with the widespread uses of radiation, there is an increasing tendency to endeavor to control its hazards through legislative means. Legislation can be good or bad; whether it is one or the other will depend to a considerable extent upon the action and interest of our scientific and technical organizations.

Today, radiation rules, laws, and regulations are being developed rapidly. With any law or regulation, restriction is apt to follow. This can be harmful if such restrictions are allowed to interfere materially with the normal progress of science and industry. These problems will undoubtedly come to your attention more and more. I want to emphasize the importance of keeping a close watch on these developments, and when necessary, being willing to devote some of your time and energy to assure that regulations are sensible, useful, and yet at the same time nonrestrictive.

The term "radiation-protection standards" is used somewhat loosely, and yet at the same time there is probably very little real uncertainty as to the broad meaning of the term. One normally thinks of a standard as being something rather firm, rather well understood, inflexible, accurately known, and reproducible. When a standard is mentioned one immediately thinks of something like the standard meter-bar, carefully locked away in a vault and resistant to the changes of time; or of some accurately measurable quantity, such as the ohm or volt.

However, in the field of radiation protection standards, there are many unknowns and many uncertainties. They involve a great many assumptions that may have to be changed from time to time. In fact, it would not be oversimplifying the case to state that our protection standards are essentially protection goals or objectives. Where it is possible to develop numbers to assign to some of the standards, these numbers are really more in the nature of a means to achieve some goal than the goal itself. Even the goal itself is difficult of definition. It is primarily to determine the limits of radiation exposure to which the individual, or whole population, can be exposed without encountering risks incommensurate with the benefits to be expected from its use. Standards of safety go back directly to the individual who will be exposed to radiation. For this it is necessary to determine how much radiation he can absorb without injury to himself or to his progeny. Knowing the complication of the human being as an organic structure, and the great difference in sensitivity between individuals, it is very easy to see why such a determination immediately becomes an extremely difficult problem. To point up the difficulties, it may be helpful to review rather briefly some of

[1] This paper is the substance of the Lester Honor Lecture, delivered before the Society for Non-Destructive Testing and published in full in the journal of that Society. Their permission to reprint this part is acknowledged with appreciation.

the early background and philosophy leading to the development of standards of radiation protection. Much has been written on this subject and therefore only the high spots will be touched upon (*1*, *2*, *3*, *4*).

Radiation was recognized as a potential hazard to health soon after its discovery. Serious efforts to understand and curtail radiation exposure to individuals were not begun seriously until the 1920's. One should bear in mind that it was not until 1928 that the world had a uniform and acceptable unit of adiation dose, namely the roentgen. Consequently, radiation-protection efforts and protection standards were, of necessity, on a purely qualitative basis. Until 1928, most radiation treatments were expressed in terms of fractions of erythema dose—the amount of radiation that would cause a defined reddening of the skin. This in itself was a very uncertain factor, since it depended upon the energy of the radiation, the time over which it was delivered, the size of the irradiated field, and the amount of backscattering, not to mention the individual's idiosyncrasy with regard to radiation sensitivity. It was not until the mid-thirties that stray radiation exposure was measured quantitatively, making it possible to put radiation-protection standards on a reasonably firm quantitative basis. Interestingly enough, later efforts proved that these early standards were not grossly wrong.

The first protection standards, if such they could be called, were in terms of thicknesses of lead that were required to be interposed between a radiation source and an individual to assure his safety (*5*). Graded thicknesses of lead were recommended on the basis of the voltage applied across the X-ray tube. No account was taken of the hardness of the radiation, distance factors, origin and history of the scattered radiation, time of exposure, etc. The standard barriers were based on supposedly "average" conditions. The following of this principle of protection, while a substantial achievement, nevertheless resulted at times either in underprotection or costly overprotection.

One could venture the suggestion that if there is any basic standard of radiation protection it would be what is now referred to as the maximum permissible dose, or maximum permissible exposure of an individual. By maximum permissible exposure is meant the amount of radiation to which the whole body of an individual can be subjected over the period of his adult lifetime, without producing in that individual any detectable harmful effects (*6*). For the occupational exposure of an individual to radiation, such a standard might be adequate, but as explained below in more detail, there should be an additional basic standard relating to the average exposure of the entire population; for genetic reasons.

Parenthetically, it should be remarked that the old term "tolerance dose" that prevailed for many years is a complete misnomer. There is no such thing as a "tolerable dose of radiation." No radiation effects, other than for the treatment of disease, are known to be beneficial to man. Any radiation exposure received by man must be accepted as harmful. Therefore, the *objective should be to keep man's exposure as low as possible and yet at the same time, not discontinue the use of radiation altogether*.

The big problem is to obtain some quantitative idea as to the amounts of radiation that can harm the human being.

As already mentioned, early permissible exposures were expressed in terms of the erythema. This followed as a result of a very few observations on a very few people who had been overexposed to radiation, under conditions where there was some crude idea as to the amount of radiation involved (*7*). As a result of this, numerous proposals were made, and for a time served a useful purpose. For example, Mutscheller proposed as a "tolerance dose," $\frac{1}{100}$ of an erythema dose in 30 days (*8*). Others reduced this to $\frac{1}{1000}$ of an erythema dose in 3 days. Sievert independently proposed $\frac{1}{10}$ of an erythema dose per year (*9*), which was not appreciably different from Mutscheller's value.

Various attempts were made to place the erythema dose on a sound physical basis. Glocker and Kaupp (*10*) described a tolerance dose as that radiation level which would give just barely visible fluorescence observable in a completely darkened room by dark-adapted eyes. They also described it as barely visible blackening of a "duplitized" X-ray film after an exposure to radiation of one hour. Mutscheller's formula, a simple but very inaccurate one for com-

puting erythema doses, was developed for a given distance from the X-ray tube for the direct beam. The number of erythema doses was given by the number of milliampere minutes, divided by 25 times the square of the distance.

Knowing as much as we do now about radiation, these standards look pathetic indeed and yet they marked important milestones leading to our more accurate understanding of the problem. When one realizes that, on the basis of today's knowledge, an erythema dose under given conditions may vary from 270 to 1,000 roentgens over a range of 100 to 1,000 kilovolts, some visualization of the vast uncertainties in the early work might be had (*11*).

By the early 30's such works as just mentioned were correlated and reconciled by the free use of safety factors—or just factors. Germany proposed the first quantitative expression of a permissible dose measured in roentgens, arriving at a figure of 10^{-5} roentgen per second as their so-called tolerance level of radiation. In 1934 it was possible, for the first time, for the International Commission on Radiological Protection to express permissible exposure in terms of roentgens (*12*). The value then chosen was $^{2}/_{10}$ roentgen per day. In the United States in 1936 a somewhat lower level, namely, $^{1}/_{10}$ roentgen per day was adopted (*13*). This lower value was in part a result of the belief that there was not an adequate safety factor in the international recommendations.

It will be noted that the permissible exposures mentioned above were integrated over varying lengths of time, ranging over a period of 1 second to one year. Although precise information on radiation recovery is lacking, it is undoubtedly true that a given dose of some roentgens received in a period of a few minutes is probably more harmful to the individual than the same dose distributed evenly over a year's period of time. Therefore, while the various proposals for permissible exposure appear to reduce numerically to the same quantity, they were not, in fact, biologically equivalent.

Additionally, there is a serious administrative problem involved, when one compares the integration of a dose given over a few seconds or over a year. For example, according to the early German proposals, if at any time an individual is exposed to more than 10^{-5} roentgens in any one second, he would have exceeded his permissible dose rate even though this might only occur once in a year. This, of course, is nonsense. On the other hand, the problem of integrating a dose over a year's time could also present serious difficulties, depending upon the particular technique used. A person might be heavily overexposed during an early period in a year, yet this might not be detected until it was too late. It was through consideration of such reasons as these, that integration over a period of one day was adopted in the mid-thirties.

In 1946 the National Committee on Radiation undertook an intensive review of the whole problem of permissible dose. This review was instigated by the fact that, during the Manhattan District days, a tremendous amount of experimental and biological research had been carried out for the purpose of assuring safety to radiation workers; new biological data had become available. It was quickly realized that the value of $^{1}/_{10}$ roentgen per day, as used in this country, provided only marginal protection. There was increasing evidence leading the committee to believe that the value should be lowered. At the same time it was decided to review the question of the period over which the dose would be integrated. For technical, as well as administrative reasons, it appeared that integration over one day was unnecessarily restrictive. Integration over about 1 month appeared to be more reasonable and a compromise was finally reached at one week. The committee arrived at the recommendation of $^{3}/_{10}$ roentgen per week as the permissible whole-body exposure to gamma rays and moderate and medium energy X-rays. This value has since been adopted internationally (*16*).

It might be pointed out that integration over a week still presents an occasional source of administrative difficulties, particularly in large nuclear industry operations. In such operations the situation occasionally arises where a person may be exposed to more than his weekly allowance, and yet would not receive any serious additional exposure for many weeks thereafter. It seemed improper to penalize either the worker or the work because of such occasional over-

exposure. Therefore, a modified standard was recommended to take care of such situations.

According to this, under special circumstances, a person's exposure may be integrated over a period of 13 weeks (or ¼ of year) during which period he may be allowed a total exposure equal to that which might have been received at the normal maximum rate for 10 weeks. Thus the 13-week integration carries with it a penalty of about 23 percent. In other words, if a person receives his total dose for the longer period all at once, his permissible dose for that period is only $^{10}/_{13}$ of what it would otherwise be. There is little quantitative foundation for this concept, other than the general belief, as already mentioned, that a dose distributed over some appreciable time is less harmful than the same dose taken all at once.

Thus far the problem of radiation exposure to the individual has been considered only under essentially occupational conditions. There are a number of other conditions that have to be considered. These will be enumerated briefly and some of them will be taken up in more detail later. Radiation effects on human beings may be more or less significant depending upon many factors. For example, there is increasing evidence that irreversible genetic damage may result from exposure of the gonads to *any* amount of radiation. All such exposure is cumulative. This implies that an exposure that may not produce any harm to the individual himself, may be passed on through the genetic chain to the descendants of this individual. It is impossible to neglect the possibility that such damage may occur.

It also appears that individuals are more susceptible to radiation damage during their embryonic stage. For this reason it is considered advisable to restrict as far as possible the exposure of pregnant women, especially during the first few months of pregnancy.

Exposure of children under 18 years of age should also be held to a minimum, although there is no positive evidence that they are more susceptible to radiation damage than an adult. On the other hand, the permissible exposure for adults takes into consideration the possible genetic damage during their lifetime prior to the conception of their children. To allow children under 18 to receive the same exposure would start them at a disadvantage were they later to go into radiation work and then to receive the maximum permissible exposure.

As far as a potential parent is concerned, the important period of his life with regard to radiation exposure is that time up to the conception of his last child. On the average this would not be higher than the age of 45 years. For this reason the permissible exposure to persons over 45 years of age may be doubled. This might provide some degree of flexibility in certain industrial operations, but some say that the differentiation of people under 45 and over 45 years of age is an administrative headache.

An additional problem, and one that will be dwelt upon in more detail later, concerns the exposure of large population groups. Take for example, a large city supplied by a single source of water. If this water becomes contaminated by radioactive waste, a very large number of people might receive a small but continuing dose of radiation. Depending upon the nature of this contamination, the radiation might reach the gonads. In this situation a small genetic damage might result to a large number of people where the possibility of crossbreeding is large. This could be deleterious to the well-being of a considerable number of people. Hence standards for the protection of a large population group may have to be substantially different from these for a relatively small number of radiation workers, each receiving individually higher doses of radiation.

Thus far the discussion has dealt, in the main, with the standards of protection from sources outside of the body. When the radiation sources get into the body the problem is vastly more complicated and the overall difficulty of reaching our standards is much greater. Here again the basic protection standard relates back to the maximum permissible dose of $^{3}/_{10}$ roentgen per day. However, in the case of internal emitters, one must consider the particular organ, or organs, that may be sensitive to the radiation. This involves an intimate knowledge of what happens to every chemical element, once it is inside the body. For example, certain elements such as radium, plutonium, and strontium are what is known as bone seekers.

By one means or another a fraction of the material entering the body eventually ends up firmly fixed in the bone where it can create damage. Another material, such as iodine, will tend to concentrate in the thyroid. Generally speaking, there are definite organs in the body which are more susceptible than others to radiation damage, both because of the nature of the organ and the tendency for that organ to concentrate radioactive material.

Depending upon the nature of the material, and its means of entry into the body, varying fractions will be eliminated by normal processes, while the remainder is taken up by the body organs. It is necessary to determine these relative proportions. The degree of damage to an organ by a given quantity of radioactive material also depends upon the biological half-life which in turn includes the physical half-life of the material. By biological half-life is meant the time that it takes to reduce the quantity of the material in the body to one-half of its initial value through the process of body elimination and radioactive decay. Thus an isotope that has a short half-life, and is also quickly eliminated from the body, is relatively non-hazardous. Such an isotope is sodium-24. On the other hand radium and plutonium are both elements having extremely long half-lives, and at the same time a low elimination rate, together with a tendency to concentrate in the bone. The former (sodium-24) is relatively non-hazardous, whereas the latter are very hazardous.

In arriving at permissible exposure levels for radioactive elements within the body, the following factors, among others, have to be taken into consideration.

1. Quantity of radioactive material taken into the body.
2. Initial body retention.
3. Fraction of material going from blood to critical body tissues.
4. Radiosensitivity of tissue.
5. Size of the critical organ.
6. Essentiality of the critical organ to proper function of the body.
7. Biological half-life.
8. Physical half-life.
9. Energy of the radiation produced by the isotope.
10. Specific ionization and the attenuation of that energy in tissue.

Once all the biological and physical factors are known, it is then possible to work backwards to determine the concentrations of radioactive material in air or in water that may be taken into the body without resulting in damage to the critical organ or tissue. By this means it has been possible to develop a series of values for the maximum permissible concentrations for radioactive isotopes in air and in water, which can be described as standards of radioactivity level. Such levels for about 100 radioactive isotopes have been worked out and agreed upon by the National Committee on Radiation Protection and have since been adopted internationally (*14*).

A slightly different problem, resulting in a different series of standards, develops when one considers the possibility of a single or occasional intake of radioactive material during a lifetime, and where little or no other exposure to radiation has been received. For these circumstances, very much larger quantities of radioactive material may be taken into the body without serious harm. For situations of this nature, it is possible to group the radioactive elements into four categories that are representative of their single-dose hazard. The elements may be placed in these categories depending on the amount of safe single dose of say 1, 10, 100, or 1,000 microcuries (*15*).

This has also been found to be a convenient grouping for legislative purposes. Since there is obviously little point in trying to control and regulate a quantity of radioactive material that is not harmful if taken in its entirety, there is no reason why it should not be exempted from control. Unrestricted use of the established quantities should be allowed in accordance with the groupings just mentioned.

Standards necessary for the control of the disposal of radioactive wastes, present still a different problem. Having once decided what would be a safe permissible concentration in air and water, the next problem is to determine the conditions under which such concentrations may occur, in situations where the air or water may be taken into the human system.

An additional complication arises, because of the possibility that radioactive wastes may by some means or another enter into our

food chain. For example, radioactive wastes discharged into the air or river may be taken up and concentrated by animal or plant life and eventually get back into the human system by way of the food supply. In spite of the extensive studies made of this problem, it is still necessary to provide very substantial safety factors with regard to the permissible amounts of radioactive material that may be discharged into the public domain. It must also be borne in mind that once radioactive material has been dumped outside of a controlled area, its future from there on is largely beyond the control of man. To allow for such uncertainties, permissible concentrations of radioactive wastes are not allowed to exceed $\frac{1}{10}$ of those allowed for continuous occupational exposure (*15*, *16*). This probably represents a very conservative standard of protection, except under the most exceptional circumstances. It is, of course, the single exceptional circumstance that dictates caution.

Additionally, for regulatory purposes it is necessary to establish standards of contamination that will apply under conditions that can be subject both to the control of the user and to inspection by an enforcing agency. This may at times lead to seemingly harsh requirements but from a legal or regulatory point of view, one does not see any easy way of avoiding the situation.

For example, in one early proposed regulation it was specified that the radiation level at any point outside of an installation could not exceed 30 milliroentgens per week, or $\frac{1}{10}$ of the level that is allowed for continous occupational exposure for 25 years of a person's lifetime. The argument here is reasonable to the extent that the point of discharge is the last point at which the installation can control its discharge. Once the material is outside the installation, it is beyond their control and probably even beyond the ability of an inspection agency to evaluate. The possibility exists, however remotely, that by some quirk the material may become dangerously concentrated at some point in the chain of events leading to human consumption.

It is for such reasons that standards for radioactive contamination in uncontrolled areas, namely the public domain, must not be allowed to exceed $\frac{1}{10}$ the maximum permissible amounts for continuous exposure. This applies particularly to situations where large population groups may be exposed to radioactive contamination under conditions which are completely and totally beyond their control (or for that matter any control). The situation is somewhat ameliorated in the recommendations of the National Committee on Radiation Protection by allowing such population exposures to be integrated over a year instead of a week (*6*, *15*).

Thus far the discussion has covered a whole series of standards for radiation protection, namely, protection of the whole body or parts of the body from external radiation sources, protection for occupational workers, continuing exposure to radiation either internally or externally, protection of individuals against large single internal doses of radiation, protection of large populations, standards for animal and vegetable life, for persons over 45 and under 45 years of age, for persons under 18 years of age, and for pregnant women. It is very clear from this sketchy outline that the problem is not a simple one and it is further clear that the standards discussed here must be subject to change—and probably frequent change. Radiation is unquestionably here to stay with us, and it is important that we learn how to use it under conditions which are both safe and economical. The price for unnecessary safety is high, but this is the price that will have to be paid until the problem is better understood.

There is however, one large outstanding problem in standards of radiation protection that has only been touched upon, namely, the genetic problem and the exposure of large population groups. This problem directly influences our whole philosophy with regard to radiation protection, in that the genetic effects of low-level exposures of a large fraction of the population may be the prime determining factor in deciding upon the permissible dose for all persons. As already indicated, the chief consideration up to this point has been in deciding upon the maximum permissible dose of radiation to the individual.

The current maximum level of permissible exposure for the individual rests on the philosophy that exposure at this level throughout his adult lifetime is believed un-

likely to cause him detectable bodily injury at any time during his lifetime. Based on this major premise, the present permissible exposure levels are acceptable both from the plant and the individual's viewpoint. They do not appear to involve an unreasonable working risk. Such levels are, however, also based on the additional premise that only a small portion of the world population will be so exposed up to the close of the reproductive lifetime of any individual.

On the other hand, where large population groups may be exposed, the preservation of the genetic balance of the population may require that the exposure per individual, averaged over the whole population, be limited to only a very small fraction of the individual occupational exposure. Up to the present, most of the pertinent data is from animal rather than human experiment, yet we are forced to tentatively accept the animal data and apply it to man.

For purposes of discussion, consider Muller's statement that an exposure of 80 roentgens to the gonads would double the natural mutation rate and that such exposure repeated generation after generation might seriously upset the genetic equilibrium (*17*). Presumably this would be genetically unacceptable in view of the doubly-heavy genetic load thrust upon the unexposed population, and in view of the present trends in reproductive practices. Muller suggests a maxium exposure per individual per reproductive lifetime of 20 roentgens, again averaged over the whole population. This would result in an increase in the mutation rate of only 25 percent. Other authorities have arrived at figures as low as 3 roentgens as a permissible average "lifetime" exposure. Either figure is much lower than the presently accepted individual occupational exposure limit which may be as high as about 400 roentgens in a lifetime.

If we are to adhere to the 20 roentgens averaged for the whole population, not more than 5 percent of the people could be permitted to receive a full occupational exposure of 400 roentgens to the gonads. For the United States this would be some eight million persons—a figure unlikely to be attained for many years to come. One should also consider that most radiation workers do not receive exposures over periods as long as 25 years, and that in fact only a very small number receive more than about one-third of the permissible exposure. On the other hand, there must be added to this, the medical and diagnostic exposures wherein radiation may reach the gonads.

Before facing the problem of determining how much additional radiation may be received by nonoccupational population groups, it is essential to make a careful evaluation of existing exposure patterns. Since the prime consideration will be genetic effects, such exposure evaluation should be limited to the gonads of persons before the close of their reproductive lifetime.

Concurrent with such a study should be certain sociological investigations. Reproduction habits will play an important role, and these will vary markedly depending upon such factors as race, education, inbreeding within certain geographic limits, etc. In averaging the exposure of population groups, erroneous results might be obtained by equal weighting of, say, the population of New York City and the population of the rest of the state, of which a considerable portion is rural.

Since the size, distribution, and nature of a population group may influence the pattern of crossbreeding, it may be worthwhile, even within a single country such as the United States, to consider very different average exposures for different parts of the country. Such differentiation might present almost impossible administrative problems because of population movements, yet there are relatively large and different population groups between which crossbreeding is negligible. This same consideration, however, will be likely to necessitate lowering of average exposures in some areas where inbreeding is high within a population group that remains somewhat static in location. There will undoubtedly be special problems of this sort in certain areas and it might be worthwhile to treat them specially, rather than inflict unnecessarily low permissible exposures over the country as a whole. Analysis of individual situations, while costly to perform, may nevertheless be sound economy in the end.

We have to face the high probability of an enormous growth in the uses of nuclear energy. It is doubtful whether anyone can

accurately predict this for the next 20 or 100 years. Looking backward, the enormous changes in our civilization brought about by technological advances in the past four or five generations, indicate the futility of planning ahead in detail for a similar range of time. This does not imply, however, that we should stand passively by and let nature take its course. There is much that can be done in preparation for the future.

As the invention of the internal combustion engine revolutionized the world's economy, so may we expect controlled nuclear energy to do so again. The engine has brought vast improvements into our material way of life. One wonders, had it been anticipated in 1910 that the engine would be responsible in less than half a century for the deaths of over a million people on United States highways alone, if there would have been a hue and cry to curtail its further use. In spite of the fact that we somehow accept this carnage, we might have been able to hold it down had the problem been attacked while automobiles were in their infancy. But who in 1910 could have predicted today's automobile industry?

With nuclear energy the situation is even more complicated—and vastly different. We know a great deal about its potential hazards and to some extent how to cope with them. In addition, we have some time in which to solve the special problems. However rapid the growth in the use of nuclear energy, there is still some leeway that will allow us to proceed with technological developments before we outrun our practical limits in the methods of protection. The greatest and most serious limitation is that imposed upon us by genetic considerations. Our most substantial advances in knowledge of genetics have been within the past two or three decades—a very short time. The results of this knowledge have been a major consideration in the discussions leading to our present concepts of permissible dose, yet we have avoided any attempts to rigorously define the genetic limitations. (It is presumed that, to be safe, the geneticists, from whom we get our information, assume the most pessimistic conditions.)

At some time, but probably not within the immediate future, man will be faced with making an inescapable decision. At what point may the advantages of atomic energy be offset by the disadvantages to the future man? And who will have the abundant wisdom to recognize that point and do something about it? Will it be known, in time for such a decision, just what radiation may do to man's future? I believe that at some point a decision involving an educated gamble with man's future will have to be made, and history of the past indicates that such a decision may be made on the less—rather than the more—conservative side. That decision does not need to be made tomorrow or probably for some years. In the meantime, our present pace can be continued with relatively little risk. But in that same meantime, we should start to condition our thinking for a change in philosophy with regard to radiation exposure. On the basis of today's knowledge of ourselves, we may be expected to show a willingness to accept more rather than less radiation exposure insofar as its effects 10 or 20 generations hence may be felt.

In this same time, we should also devote our every energy to keeping radiation exposure of persons to the minimum compatible with reasonable progress and good sense. Through education and the dissemination of wisely chosen information, much can be done to improve the present situation in matters of radiation protection, without at the same time fettering a source of great benefit to mankind. The better the ordinary job of protection is done today, the longer is the fateful decision on man's future postponed.

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NBS-AF PANORAMIC X-RAY MACHINE

Research at the National Bureau of Standards has produced an X-ray machine that rapidly takes a single panoramic X-ray picture of the entire dental arch. Developed by the Bureau in cooperation with the U. S. Air Force Dental Service and the USAF School of Aviation Medicine, the panoramic X-ray machine should be particularly useful to the armed forces in making full-mouth dental X-ray surveys of inductees on entering and leaving the service. It will save much of the time required by present techniques, in which up to 14 small films are exposed separately for a full-mouth survey. The device should find application wherever large numbers of people are examined for dental defects, and its principles can be applied to radiography of other parts of the body.

In the NBS-AF panoramic machine, the film is placed outside the patient's mouth and is exposed by passing a narrow beam of X-rays through his head from the rear. A panoramic X-ray picture of all the teeth and associated structures is obtained on a single 5 x 10-inch film in about 40 seconds. Thus the problem of handling many small film packets during exposure and processing is eliminated. At the same time more comprehensive radiographs are produced, giving more general diagnostic information than do conventional full-mouth X-ray surveys.

For several years investigators in both the United States[1,2] and Finland[3] have been seeking to develop a rapid, practicable method for making panoramic X-rays of the entire dental arch. However, the procedures that have been tried have been rather cumbersome—they required rotation of the patient or fitting films inside the mouth. Because of the large number of full-mouth surveys that must be made of military personnel, particularly at induction stations, a rapid, dependable method was needed to replace conventional radiographic techniques. Funds for research in this field were therefore provided by the Air Force. As a result, a more flexible and simple panoramic machine was developed by Col. D. C. Hudson, NBS guest worker from the U. S. Air Force Dental Service, and J. W. Kumpula of the Bureau staff, with the cooperation of members of the NBS electronic instrumentation laboratory.

In this machine, an X-ray source and film holding device follow semicircular paths on opposite sides of the patient's head. The film holder travels in front of the patient, the X-ray source behind him. Movement of source and film is so coordinated that only those structures of the dental arch desired in the finished film are sharply projected while other overlying structures are not.

The X-ray source and film holder are suspended from opposite ends of a horizontal arm that rotates about a central vertical axis. A narrow beam of X-rays emerges from a slit in the exit cone of the X-ray source, passes through the subject's head, and enters a corresponding slit in the film holder just beyond his teeth. Meanwhile, the film, in a carrier within the holder, travels horizontally in a direction opposite to that of the holder and at such a rate that an X-ray shadow of each successive tooth falls on successive areas of the film.

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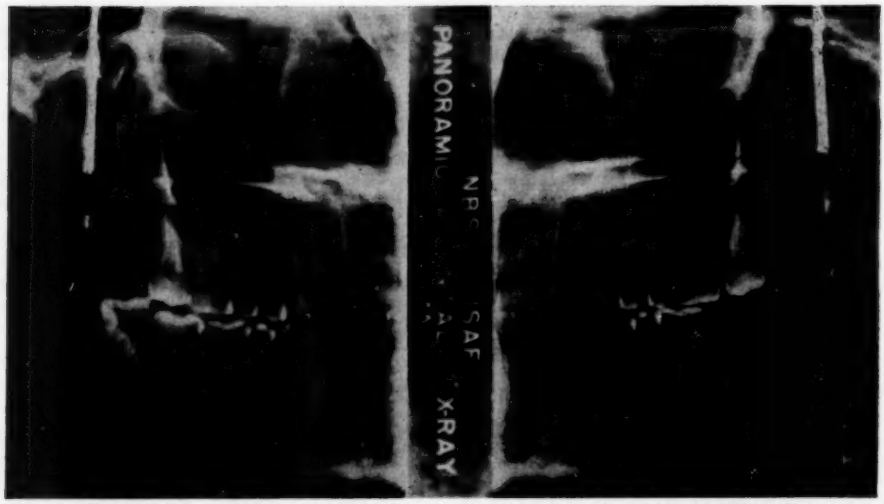

Fig. 1.—Typical X-ray photograph of the human dental arch produced by the NBS-AF panoramic X-ray machine. Parts of the nasal sinuses and other bony structures of the head can also be observed. This radiograph was made on a wax phantom head. The strip in the center normally carries the patient's name and date.

To avoid unwanted X-ray shadows from overlying bony structures, the X-ray beam is made to enter the patient's head largely through the soft tissue between the vertebral column and the bone of the jaw—first from one side, the from the other. As the system rotates, the axis of rotation of the X-ray beam is in this soft tissue, which is much more transparent to X-rays than are the harder structures. Thus, since the X-ray beam is in constant motion, shadows cast by intervening bone and other tissue between the point of entry and the dental arch move across the film too rapidly to obscure detail in the exposure. As a result, clearer lateral jaw radiographs can be obtained than with conventional techniques.

A simple mechanical system automatically varies the rate of film travel to conform to the size and shape of the human dental arch. This device consists of a cable wound about a cam which is curved in the shape of an average dental arch. The free ends of the cable are connected through pulleys to the film carrier. As the horizontal arm rotates at constant speed, the film moves at a rate determined by the curvature of the cam surface at the point where the cable is leaving the cam. The cam is fixed at the axis of rotation, while an electric motor rotates the arm supporting the X-ray source and film holder. A central panel controls the motor as well as X-ray voltage and current.

The first model of the panoramic X-ray machine is now undergoing performance studies. In this model the patient sits in a dental chair beneath the rotating arm and is positioned by means of a chin support pivoted from a stationary point on the machine. The chin support serves to steady the head and to place the dental structures in the proper position for best projection onto the moving film. Future models may be so constructed that the subject need not sit but may be radiographed in a standing position. This will further reduce the time needed for a dental survey.

With the cooperation of the Naval Medical Research Institute, a study[4] was made of the radiation levels produced by the panoramic X-ray machine at points in and about a phantom head constructed of tissue-equivalent wax molded upon an adult human skull. By the use of small ionization chambers—about 30 mm³ in volume—it was found that the panoramic device produces lower radiation levels than the conventional 14-film intraoral technique produces at corresponding points. The reduction in radiation received by the patient is due to the small area covered by the beam of X-rays during panoramic exposure and the fact that no overlapping occurs.

[4] *Ionization chambers for radiation data during dental x-ray exposure*, by D. C. HUDSON and J. W. KUMPULA, Armed Forces Med. Journ. **6**: 1131. August 1955.

ENGINEERING.—*The influence of space flight on engineering and science.*[1] MILTON W. ROSEN, Naval Research Laboratory. (Communicated by C. H. Page.)

Within the past few years many scientists have predicted seriously and confidently that human beings from the earth would, in the foreseeable future, travel to the moon and the nearer planets. The ranks of those who would dispute this prospect are diminishing rapidly. Although much of the progress is still guarded by military necessity, space flight is emerging as an activity in its own right—one that can command the efforts of many engineers and scientists.

In the United States the exploration of the upper atmosphere, the frontier to space, is a vigorous and continuing activity. Pilots of rocket aircraft have experienced conditions approximating those in free space, if only for a few minutes. The effect of space flight upon the human organism is being investigated—the U. S. Air Force maintains a Department of Space Medicine.

There is an international organization devoted to the promotion of space travel and there are space flight societies in 23 countries. Numerous journals exist wholly or in part for the publication of papers on astronautics and its allied fields—notable among these for the quality of its articles is the Journal of the British Interplanetary Society.

I shall try here to explain how the present state of affairs came about and also to forecast what might be the future influence of man's effort to travel in outer space.

The ancients, except for a few rare individuals with greater insight, conceived of the world as an enclosure; they stood upon the earth at the bottom and gazed upward at a blue ceiling upon which a multitude of lights, a few great and many small, seemed to move under the influence of an unseen hand. The atmosphere filled this enclosure and it was believed that if man had wings he could fly to the ceiling and determine the source of the lights. If a few philosophers guessed more nearly at the truth, certainly the average man had no better conception of the universe than the fanciful picture just described. There could be no valid idea of space flight until Copernicus, Kepler, and Galileo placed the earth in its true relation to the universe and at the same time gave dimensions to space.

When at last the moon and the planets were found to be material bodies not unlike our earth, it was possible to ponder whether the immense separating distances could be traversed by man or any man-made device. The situation was made even more discouraging when, in 1686, Newton (*1*) defined the nature and the magnitude of gravitational attraction. If previously there had been some fanciful hope of visiting celestial bodies, now surely it appeared that man was destined to remain forever a prisoner of his own planet. In view of the great distances, it seemed unlikely that the atmosphere could extend through interplanetary space, and any suspicion that it might was laid to rest when Torricelli's barometer was carried to a mountaintop and taken aloft by the early balloonists.

Although Newton brought man face-to-face with one formidable aspect of the problem, namely gravity, he also provided, in his three laws of motion, the key that would unlock the door to space. The fundamental equation of rocket action in free space and hence of space flight:

$$V_b = C \cdot \log_e \frac{M_o}{M_b}$$

where V = velocity of rocket at end of burning
C = velocity of exhaust jet
M_o = initial mass of rocket
M_b = mass of rocket at end of burning

is derived by integrating Newton's third law of motion. Nevertheless, more than two centuries would pass before anyone performed the integration or realized that the simplest embodiment of Newton's third law, a rocket, is the only machine capable of propelling itself in a vacuum.

While science gave no solution, and, indeed, many scientists despaired of finding one, the dream of travel to celestial bodies

[1] A lecture delivered before the Washington Society of Engineers, November 16, 1955.

was kept alive in fiction. One of the earlier references to rockets for propelling a space ship is found in the writings of Cyrano de Bergerac (*2*), but it is doubtful that Cyrano understood the rocket's essential role. Perhaps the most famous novel about space travel is Jules Verne's *From the Earth to the Moon* (3). Although Verne had expert scientific advice, he chose an impossible means of projecting his space ship—it was fired from a long cannon sunk in the ground. Probably Verne knew that no human could survive the acceleration of his projectile and that the projectile itself would disintegrate under the tremendous forces imparted to it. Yet, millions of readers believed his story; many thought it had actually been accomplished—so great was his art—and he created a myth that had to be destroyed before any scientific progress could be achieved. In his novel *The first men in the moon* H. G. Wells (*4*) felt no necessity for scientific rigor, and he conjured up a gravity-defying substance which he called "cavorite."

By the beginning of the twentieth century the physical sciences had advanced to the point where it was inevitable that someone would develop a valid theory of rocket action and would apply the theory to the problem of escape from the earth. The task was accomplished by three men working independently in three different countries. The three had much in common—they were teachers, one at a small university, the other two in secondary schools.[2] Each one pondered the problem for many years before committing his findings to publication. But what is most important, all three were motivated by the desire to explore interplanetary space and presented their findings with conviction, even though they were regarded by most of their contemporaries as prophetic dreamers. The men and their works are now well known. They are:

ZIOLKOVSKY—*The exploration of cosmic space by reaction machines*, 1903.

GODDARD—*A method of reaching extreme altitudes*, 1919.

OBERTH—*The rocket into interplanetary space*, 1923.

[2] One, Oberth, became a teacher after his fundamental work had been completed.

Any one of these three publications, had it been widely read and accepted, would have sufficed to lay the groundwork for space travel, because each man clearly understood and asserted the following fundamental concepts:

1. That escape from the earth is possible by the application of a moderate acceleration over a substantial period of time—at least several minutes.

2. That such acceleration can be produced in a vacuum by a rocket.

3. That the rocket must (a) have high thermal efficiency (i.e., high velocity of the ejected matter) and (b) consist mainly of propellant material (i.e. have a high ratio of fuel weight to total weight).

4. That high thermal efficiency would be obtained most readily from the chemical combustion of liquid fuels.

Ziolkovsky (*5*) started by examining Jules Verne's cannon and also the balloon as a means of reaching very high altitudes. Both approaches died quickly under mathematical analysis. He proceeded next to the rocket and developed the fundamental equation previously noted. Realizing that energetic fuels were required, he determined from thermochemical calculations the heat release of various liquid combinations. When he computed the velocity that could be attained, in theory at least, he realized it was sufficient for escape from the earth.

Goddard (*6*), alone of the three, proceeded from experiment to theory. Using smokeless powder in a heavy-walled steel combustion chamber he produced a jet velocity of almost 8,000 feet per second, a sevenfold improvement over ordinary rockets and the highest velocity of matter attained up to that time outside of electrical discharge tubes. Also, he proved by tests in a vacuum that a rocket does not produce its force by pushing on the air behind it, a fact he knew from basic physics, but that he felt had to be demonstrated. He observed correctly that the jet velocity was greater in a vacuum, but he attributed it erroneously to more efficient ignition. Although Goddard did not turn to liquid fuels until after his basic paper was published, he achieved the first flight of a liquid rocket, an event that took place on March 16

1926. Goddard (*7*) continued his experiments for more than two decades during which time he developed, in rudimentary form, almost every component of modern rocketry. Not one of his components would be considered reliable by present-day standards; realizing the prodigious task he had set out to accomplish, he would repeatedly add a new component before perfecting the previous one. In retrospect, it appears that Goddard was attempting, single-handed, to encompass the entire field of liquid-rocket development, a task that would eventually tax the abilities of thousands of engineers and scientists.

Oberth (*8*), in his treatise, gave the most complete theoretical analysis and carried it farther into the realm of space travel than either of the others. He stated at the outset the four propositions he would attempt to prove:

1. Considering the present state of science and technology, it is possible to build machines that could rise beyond the atmosphere.

2. After further development these machines will be able to attain such velocities that, left undisturbed in the depths of outer space, they will not fall back to the earth and will even be able to leave the zone of terrestrial attraction.

3. These machines could be constructed so as to transport human beings, probably without damage to their health.

4. Under certain economic conditions the construction of such machines might be profitable.

Oberth began by developing the theory of the liquid-rocket and describing its construction. He proceeded to discuss applications of the rocket, first as a high-altitude sounding vehicle, then as an earth satellite, and finally as a space-ship for interplanetary travel. He developed the concept of synergic (minimum energy) ascent trajectories. Without doubt, almost every later book on space flight owes much to Oberth's encompassing study.

Whereas the first quarter of this century provided the theoretical background for space flight, the second 25 years may be viewed as the period of experimental preparation. It saw the liquid-fueled rocket develop as a practical engine for the propulsion of aircraft and guided missiles. Many fuels and oxidizers were explored—a few saw widespread use. An assortment of auxiliary hardware—pumps, turbines, valves, and regulators—was developed to feed and control the rocket motor. The steering of a large rocket vehicle was mastered by means of gyroscopes and jet controls. Great progress was made in the aerodynamics of supersonic flight, in structural design, and in the use of high temperature materials.

Of the early experimenters three groups were most noteworthy. The work of Goddard as an individual has been referred to previously. In Germany the Verein für Raumschiffahrt, fired by Oberth's monumental work, undertook to develop a small workable rocket called, appropriately, minimum-rakete (Mirak—for short) (*9*). In the course of several years they made hundreds of static firings and numerous brief flights. The American Interplanetary Society drew its inspiration largely from abroad, so secretive was Goddard about his experiments. Indeed, when in May 1933, the Society finally achieved a first liquid-rocket flight, they were unaware that Goddard had progressed far beyond his first flight seven years before. It is unfortunate that the British Interplanetary Society was prevented from experimenting with rockets, a situation frequently lamented by its founder, Philip Cleator (*10*).

The V-2, whose development started in the middle of this period, was the largest single engineering advance in the field of rocketry. By applying thousands of engineers and scientists in a concerted effort, the German government was able in six years to transform the liquid-fueled rocket from a small, sputtering vehicle, capable of ascending a few hundred feet, into a giant projectile with a range of 200 miles and a velocity of one mile per second. The V-2 was a material embodiment of Oberth's ideas and, although he conceived the liquid rocket as a vehicle for space travel, he also foresaw its possible use as a bombardment weapon. Actually, he hoped that the rocket missile would be a deterrent to rather than a tool of war.

After the war the major activity leading

to space flight took place in the United States in the form of upper-air research with instruments carried in rockets. In a continuing program, scientists from government laboratories and universities explored the upper atmosphere using at first captured V-2 rockets and later, as they became available, Aerobees and Vikings. Both Viking and Aerobee were designed specifically for probing the atmosphere in the region between 50 and 150 miles above the earth. A few of the more important accomplishments of this program are noted. Knowledge of the pressure, temperature, density, and ionic content of the atmosphere has been extended up to 135 miles by direct measurements. The solar spectrum has been recorded in the far ultraviolet. X-rays have been detected in solar radiation and their role in the formation of the ionosphere has been postulated. The number and the mass distribution of primary cosmic rays have been recorded in emulsions carried aloft in rockets. Small animals, monkeys and mice, were sent aloft and their physiological reactions observed during a period of weightlessness. For nine years man had been exploring the frontier of space as a prelude to flight beyond the atmosphere.

It is always tempting to draw parallels and it might appear, at first glance, that the advance toward space flight parallels the progress of aviation, with the latter preceding in time. The same elements of progress are evident in both fields, but one can not fail to note the differences and contrasts.

Although much theoretical work had been done on fluid mechanics and experiments performed in wind tunnels and with gliders, there was at the time of the first mechanical flight no adequate theory to explain the lift of a winged vehicle (*11*). We knew that the Wright brothers' plane flew, but we could not explain why or how it flew. By contrast, the motion of a rocket, as we have seen before, was well understood before Goddard's first flight attempt. This is no paradox—it is apparent that the mathematical treatment of flight within the atmosphere is much more difficult than the analysis of flight in free space.

In both fields there was a period when development was nourished mainly by amateurs; in aviation it was the first decade of this century, for rocketry the late twenties and early thirties. In both cases the advance was given great impetus by a war; the first World War for aviation, the second for rocketry. But I doubt if there is in the history of aviation any single step forward comparable in magnitude to the creation of the V-2. Aviation has been characterized by gradual, steady development, fostered to a large extent by its economic returns as well as its military advantages. There have been several significant milestones; one of the most noteworthy was the development by Major Whittle and others of the turbojet engine, which in the short space of a dozen years has completely displaced the piston-driven propeller in high speed military aircraft and may soon dominate the field of commercial aviation.

In pursuing this rather loose parallelism I have tried to estimate what period in the history of aviation corresponds to the present status of space flight. It seems to me that we are now at a point roughly corresponding to the period before Lindbergh's historic flight across the Atlantic. The significant event we are awaiting is the first orbital flight of a manned earth satellite.

In both cases, at the time being considered, the vehicle had been developed to a reasonable degree of reliability and many flights of shorter range and duration had been made. But again, there is a significant difference. Aviation has always implied manned flight—in rocketry most of the progress thus far has been made in unmanned, automatically-controlled vehicles. Our technology has advanced to the point where we need not risk human life in experimental rocket-flights—on the road to space, instruments will always go first and will point the way for men to follow.

Prior to establishing the first manned satellite two important techniques will have to be mastered. First, there will be a period of experimentation with unmanned, instrumented satellites during which time problems of propulsion, staging, and navigational control will be worked out. The environmental hazards—cosmic radiation, meteors, solar heat (and the absence of it), and possibly weightlessness—can be evaluated.

Worldwide realization that this first problem is being attacked vigorously came when, on July 29, 1955, President Eisenhower announced that the United States would launch small instrumented satellites during the International Geophysical Year (1957–1958). By their statements in support of the President's announcement, many noted scientists attested to the feasibility and usefulness of the instrumented satellite. It is significant, also, that the United States invited international cooperation and offered to make its scientific findings available to all nations.

The second problem is the one of safe return to the earth's surface. The relative speed of roughly five miles per second between the orbiting vehicle and the earth's surface must be brought to zero. Obviously, this will be done by allowing the satellite to transfer its energy to the atmosphere. But this process must be controlled with great precision, lest the satellite absorb too much of the energy in the form of heat. Much will be learned by observing the return of instrumented satellites, but the final preparatory steps will probably involve manned flights at gradually increasing re-entry speeds.

I have placed Lindbergh's flight and the first manned satellite in juxtaposition because one has and the other will, I believe, so excite the world's imagination that future progress will be greatly accelerated. One can not say when the desired event will take place—much hard work remains to be done—but it is not uncommon for scientific achievements to precede their predicted arrival. The mechanical components, engines of sufficient power and controls of requisite precision, are within sight. If it is argued that the human hazards are great and, at present, poorly understood, let it be remembered that the first orbital flight need only be brief—a matter of several hours. In this respect the ordeal may be less prolonged than Lindbergh's flight, but certainly no less demanding upon the pilot's judgment and courage.

We have seen that although space flight is yet to be achieved, its prospect has had, in the last fifty years, an appreciable influence upon science. The greater influence by far lies in the future. The advances in our technology necessary to achieve manned flight in space and those required to exploit it can be readily delineated. But a more important result will, I believe, be the impact of space flight upon scientific thought and education.

In America today we are faced with a serious shortage of engineers and scientists even though the demand is great and the remuneration is ample. Almost every prospective technical graduate of our universities is showered with offers of employment and our newspapers and magazines are filled with advertisements for men with technical training. The most appalling aspect of this situation is that it is likely to continue for many years. A recent survey shows that the study of physics in our public high schools has been declining for more than half a century. Whereas in 1895 more than 95 percent of high-school graduates had taken a course in physics, by 1952 only 21 percent of graduates had ever studied it (*12*). For many years the increase in high-school enrollment more than offset the decrease of specialization in physics, but now the waning interest is taking its toll. Today only about half of the public schools offer a course in physics, and a quarter of these have no laboratory facilities. There is a critical shortage of science teachers, due in part to the attractions of industry, but more so to the lower status and wages accorded the teaching profession. But these factors can be remedied with sufficient effort—a deeper and more serious cause is the lack of interest on the part of our youth. Why do they turn away from a career in science? We can only grope for the answer. Perhaps they sense, better than their elders, that too much of our scientific talent is engaged in the unproductive task of developing weapons for war. Is there much inspiration to devote one's life to this end, especially when we are rapidly approaching the borderline of total destruction?

I believe that space flight might serve in no small measure to turn men's minds toward a more appealing scientific goal. As the exploits of Cabot, Drake, and Davis inspired many generations of Englishmen to turn to the sea, so may the first astronauts reawaken our youth to the romance of scientific exploration.

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NATIONAL ACADEMY MEDAL AWARDED TO DR. WATTS

The James Craig Watson Medal of the National Academy of Sciences has been awarded to Dr. Chester B. Watts (WAS), of the United States Naval Observatory, in recognition of his outstanding contributions to astronomical research. The Medal will be presented to Dr. Watts during the Annual Meeting of the Academy to be held in Washington, April 23–25, 1956.

Dr. Watts, who is director of the Six-Inch Transit Circle Division of the Naval Observatory, has been engaged during the greater part of his 45 years at the Observatory in determining positions of the sun, moon, planets, and stars. Such measurements provide the basic data for the study of the motions of celestial bodies both within the outside the solar system. Since 1934 Dr. Watts has been chiefly responsible for the Six-Inch Transit Circle. With a judicious combination of mechanical, optical, photographic and electronic techniques, he has brought the instrument to a higher state of perfection than any other of its kind. He recently designed and supervised the construction of a new Nine-Inch Transit Circle at the Observatory. In spite of his skill in perfecting his instruments, Dr. Watts remained dissatisfied with the precision of his measurements, which are based on observations of the edge of the moon's disk. The edge that we see is always irregular because of the high mountains and low valleys on the moon's surface. Also, a slightly different aspect of the edge of the moon is seen from time to time. These factors have limited the precision with which measurements could be made. About 11 years ago, Dr. Watts undertook to survey that part of the moon's surface (comprising some 18 percent) that presents itself on the edge of the moon, and to make this survey of a surface some quarter of a million miles away accurate to within about 50 feet. His survey is now virtually complete. The work required some thousands photographs of the moon, the invention and construction of an automatic photoelectric machine for tracing the profile of each photograph and drawing it on a strip of paper 30 feet long, the design and construction of analogue computers for analyzing the profiles and translating them into numerical form, the devising of means for integrating the profiles into a representation of the surface of the moon in the vicinity of the edge, and finally the development of the most readily usable form for publication of the results. The completed work will be published shortly.

The Watson Medal was established in 1874 by the bequest of James Craig Watson, a member of the Academy and Director of the Washburn Observatory of the University of Wisconsin. He provided in his will that the medal should be awarded "to any person in any country who shall make any astronomical discovery or produce any astronomical work worthy of special reward as contributing to our science."

Is there any thing whereof it may be said, See, this is new? it hath been already of old time, which was before us.—Ecclesiastes i:10

BIOCHEMISTRY.—*An observation on pufferfish toxin.*[1] ROBERT D. MACOMBER, School of Tropical and Preventive Medicine, College of Medical Evangelists, Loma Linda, Calif. (Communicated by Bruce W. Halstead.)

Many workers have observed that the skin of pufferfish contains a considerable amount of toxin. Tani (Teikoku Tosho Kabushiki Kaisha **2** (3): 1–103. 1945) reported that some skin samples extracted and assayed contained as much as 20 percent of the total toxicity of the fish. Routine screening tests in our laboratory have also demonstrated puffer skin extracts to be strongly positive.

MATERIALS AND METHODS

With the foregoing facts in mind, it was suggested that the water used in thawing frozen puffer specimens for preservation be assayed. A mouse was injected intraperitoneally with 1 cc of the water used to thaw a frozen Japanese puffer, *Fugu pardalis* (Temminck and Schlegel). It was found that the water contained sufficient toxin to kill a mouse in about 5 minutes.

After this chance observation, the experiment was set up as follows: From another frozen pufferfish of the same species, samples of flesh, liver, gonad, and skin weighing 7 grams each were assayed to determine the toxicity of various parts of the fish. For the assay, the samples were homogenized in a Waring blender with 2 cc distilled water being added per gram of samples; the skin sample being tough and leathery, was cut in small pieces with the shears prior to homogenizing. Samples were then centrifuged 20 minutes at 2,000 rpm and the supernatant liquid decanted for injection of mice, following the procedure routinely used by Halstead (Copeia (1): 1–11. 1954.)

The skin extract assayed 3.4 mouse units of toxin per gram of sample. (A mouse unit, as recommended by H. Sommer and K. F. Meyer, Arch. Path. **24** (5): 568–570, 1937, in their work on paralytic shellfish poison has been adopted for fish bioassays. The mouse unit is defined as the amount of toxin required to kill a 20-gram mouse in 15 minutes.) From data on the other samples, it was estimated that the skin contained roughly 14 percent of the total toxicity of the fish.

For the second part of the experiment another sample of skin weighing 7 grams was then removed from the fish from an area adjacent to that of the first sample. This second sample was washed thoroughly on the exterior surface with methyl alcohol. A cotton swab wet with the solvent was used to remove all traces of mucus and the skin was scraped lightly with a razor blade. The alcohol wash and scrapings were combined and evaporated to dryness under vacuum. The residue was made up to a volume of 14 cc with distilled water for assay and the residue insoluble in water was not removed. The skin, free from all signs of mucus on the exterior surface, was extracted with water and assayed.

RESULTS

The skin extract showed toxic symptoms in the mice but they all recovered. The water suspension of the residue from the alcohol wash assayed 3.05 mouse units of toxin per gram of skin sample. The alcohol wash solution, then, contained approximately 90 percent of the amount of toxin that had been present in the homogenized sample of skin extracted with water.

The results are tabulated as follows:

Water extract of skin	3.4 MU/gm of sample
Alcohol wash of exterior skin surface	3.05 MU/gm of sample
Water extract of skin sample previously washed with alcohol to remove mucus	Weakly toxic

SUMMARY

The major portion of the toxin present in the skin of the Japanese puffer, *Fugu pardalis* (Temminck and Schlegel), appears to be in the superficial layers of the skin or sufficiently near the surface to be removed with solvent wash.

[1] This investigation was supported in part by a research grant awarded to Dr. Bruce W. Halstead from the Division of Research grants, National Institutes of Health, Public Health Service, grant No. RG-(2366)C5.

BOTANY.—*An evaluation of* Benjaminia *Martius ex Benjamin.* LYMAN B. SMITH, U. S. National Museum, and J. MURÇA PIRES, Instituto Agronômico do Norte, Belém do Pará, Brazil.

Among the aquatic plants that float on the still waters of the lakes in the campo regions, there is a small species of the family Scrophulariaceae which is rather common, especially in the campos of Bragança in northeastern Pará and in the Brazilian territory of Amapá. Its roots are buried in the soil, while its stems spread over the water and bear pale rose corollas, the whole plant being entangled in a mat of other aquatics like *Pontederia*, *Salvinia*, and *Cabomba*. In the process of identifying this species as *Bacopa reflexa* (Benth.) Edwall, we were struck by its close resemblance to the genera *Benjaminia* and *Naiadothrix*, which supposedly belong to different families. To clear the title of this species it was necessary to evaluate both genera, and the situation was so confused that it seems worth while to record our findings here.

The genus *Benjaminia* Martius ex Benjamin was validly published in the Flora Brasiliensis (**10:** 255. 1847) with a single species, *B. utricullariiformis* Martius ex Benjamin, based on a single collection, *Gardner 4347*. Under it as a synonym was listed "Quinquelobus Benj. Mss."

In the same year Benjamin published *Quinquelobus* (Linnaea **20:** 316. 1847) with this note immediately after the name: "(Benjam. in Mart. et Endl. Fl. Bras. Fam. Utric. adhuc ined.)." The note would indicate that *Quinquelobus* was the earlier name and subsequent authors, notably Beck (Engler & Prantl, Die Nat. Pflanzenfam. IV. **3b:** 123. 1895) and Dalla Torre and Harms (Genera Siphonogamarum 586. 1906), evidently have jumped to this conclusion without further investigation. The evidence of priority is just as strong for *Benjaminia*, but the whole question of priority is irrelevant because the first publication of *Quinquelobus* is invalid. There is no description of the genus itself as required by Article 48 of the International Code of Botanical Nomenclature of 1952, nor can it be considered valid as a monotype according to Article 50 because it is based on four species. The four species are also invalid according to Article 51 because they were published in an invalid genus, although all but the first have descriptions.

A year after the publication of *Benjaminia* and *Quinquelobus*, Bentham (London Journ. Bot. **7:** 567. 1848) pointed out that *Benjaminia* not only did not belong to the Utricullariae (Lentibulariaceae) but was identical with his *Herpestes reflexa* of the Scrophulariaceae. Furthermore, it was based on the same collection, *Gardner 4347*. He also indicated that the four species under *Quinquelobus* belonged to three different genera of Scrophulariaceae.

Bentham's identification of *Benjaminia* has been reenforced recently by Prof. G. Erdtman, who informs us that there is no apparent similarity between its pollen and those in the Lentibulariaceae, but that there is a resemblance to certain types in the Scrophulariaceae.

Owing to the later misconceptions of Beck and Dalla Torre and Harms, the clarification of Bentham was overlooked by recent authors. Thus Pennell in making *Naiadothrix* as a segregate from *Bacopa* (Mem. Torrey Bot. Club **16:** 105. 1920) transferred *Herpestis reflexa* Bentham to it without realizing that *Benjaminia* was already available. Consequently, anyone who believes that this taxon is a distinct genus must make the appropriate combinations under *Benjaminia*. We are not taking this action because we agree with Pennell's final stand in reducing all segregates to *Bacopa* in the broadest sense (Proc. Acad. Nat. Sci. Philadelphia **98:** 83–98. 1946). Our findings may be summarized briefly thus:

Bacopa Aubl. (1775) Section **Chaetodiscus** Subsection **Naiadothrix** (Pennell) Pennell, Proc. Acad. Nat. Sci. Philadelphia **98:** 98. 1946.

Benjaminia Mart. ex Benj. in Mart. Fl. Bras. **10:** 255. 1847.

Quinquelobus Benjamin, Linnaea **20:** 316. 1847, nomen invalidum.

Naiadothrix Pennell, Mem. Torrey Bot. Club **16:** 105. 1920.

ETHNOLOGY.—*An investigation of the early bands of the Saone group of Teton Sioux.* HARRY ANDERSON, St. Albans, N. Y. (Communicated by John C. Ewers.)

Although little, if any, investigation has been made into the condition of the Saone bands of the Teton Sioux during the years shortly before and after the beginning of the 19th century, two sources are available, which provide an excellent foundation for such a study. These are the statistical tables prepared by Lewis and Clark and printed in the *American State Papers, Indian Affairs*, volume 1,[1] and *Tabeau's Narrative of Liosel's Expedition to the Upper Missouri*, edited by Anne H. Abel.[2] The term Saone was used extensively on the Upper Missouri during the period 1800–1850, when referring to the five tribes of northern Tetons, the Minneconjous, Sans Arcs, Two Kettles, Hunkpapas, and Blackfeet Sioux.

Lewis and Clark and Tabeau were on the Upper Missouri in 1803 and 1804, recording data on the Saones at a time when these tribes were beginning the process of their development into distinct political structures. Some, like the Minneconjous, had already assumed the stature of a separate tribe containing three subbands, while others like the Two Kettles and Blackfeet can not be recognized as such, and only conjectures can be made regarding their stage of development at that time. The products of such an investigation are many. In some cases, enough facts are available to permit definite conclusions to be formed, while other situations merely invite strong suppositions, and still others result in unanswerable and frustrating puzzles. Yet, the positive products greatly outweigh the negative, for in providing a clearer understanding of the beginnings, customs, and structures of these Teton tribes, we are far better able to evaluate many of their actions in later periods of their history.

The origin and meaning of the name Saone is not definitely known, for during the 1880's when the missionaries were making their inquiries concerning the Teton bands, the name had then gone out of common usage, and little reliable information could be obtained from the Sioux regarding its meaning.[3] The first known use of the name was by Truteau, who recorded in his journal that a Sioux band called "Chahony" was expected to arrive at the Arikara village late in the summer of 1795 for the purpose of trade.[4] The best study to date on Saone origins, based upon the available sources, can be found in Hyde's history of the Oglalas. It is his conclusion that the name was given originally to the northern Teton group by the southern Tetons, the Oglalas and Brules, and in some manner referred "shooting in the trees", or living and hunting in wooded areas.[5]

Both Lewis and Clark and Tabeau list the bands of the Minneconjous separately rather than including them under the general heading of Saones. This is significant in view of the fact that other material on Teton bands prepared during the 1800–1850 period classified the Minneconjous as Saones. The earliest of these, the reports and treaties submitted by the Atkinson-O'Fallon Commission of 1825, lumped the Minneconjous together with the tribes now commonly known as the Sans Arcs and Two Kettles, and had the chiefs of these three groups sign a single treaty.[6] In 1840 S. R. Riggs, the Sioux missionary, visited Fort Pierre and recorded much valuable information regarding the Tetons obtained from the fur traders. Riggs also classified the Minneconjous as "Sanoni."[7] From this and other information, it clearly appears that the Minneconjous were a part of the Saone group, but at the time of the writings of

[1] Pp. 712–715. This information can also be found in the *Original journals of the Lewis and Clark expedition, 1804–1806*, edited by Reuben G. Thwaites, **6**: 97–99. New York, 1905.

[2] Norman, Okla., 1939, pp. 103, 104.

[3] See the *History of the expedition under the command of Lewis and Clark*, edited by Elliott Coues, **1**: 101. New York, 1893.

[4] *Journal of Jean Baptiste Truteau among the Arikara Indians in 1795*, South Dakota Hist. Coll. **7**: 473.

[5] HYDE, GEORGE E. *Red Cloud's Folk:* 12–13. Norman, Okla., 1937.

[6] The Atkinson-O'Fallon report can be found in House Document *no. 117*, 19th Congress, 1st Session; the signers of the Saone treaty in the Statutes at Large, **7**: 254.

[7] RIGGS, STEPHAN R. *Journal of a tour from Lac-Qui-parle to the Missouri River*. South Dakota Hist. Coll. **13**: 340.

Lewis and Clark and Tabeau, they were so numerically superior to the other Saone bodies that they were given a separate listing.[8]

A second significant factor appears after a careful examination of the Minneconjou material; of the three bands which made up the tribe around 1800, none can be recognized in the Minneconjou tribal circle obtained by J. Owen Dorsey some 80 years later.[9]

THE MINNECONJOUS

Lewis and Clark	*Tabeau*
Min-na-kine-az-zo	Minican-hojou
Wan-nee-wack-a-ta-o-ne-har	Tacohiropapais
Tar-co-eh-parh	Waniwacteonilla

The first group, the Min-na-kine-az-zo of Lewis and Clark, or the Minican-hojou of Tabeau, is undoubtedly the parent body of this tribe, the Original or True Minneconjou band. According to the explorer's list, the chief of this band was Wock-ke-a-chauk-in-dish-kah or Thunder Ring (*wakinyan*, thunder and *cangleshka*, a hoop or wheel, something round). An explanation to an entry in the High Hawk winter count[10] confirms that the Minneconjous had a prominent head man named Thunder Ring shortly before the end of the 18th century. Because of his leadership over the parent band, he is named by Lewis and Clark as being the head chief of the Minneconjous.

Unfortunately, no detailed list of Sioux bands is available for the period between Lewis and Clark's and that of Thaddeus Culbertson's, dated 1850.[11] By the author's own admission, the Culbertson list is not complete, and no mention is made on it of the True Minneconjous. However, the material collected by F. V. Hayden in 1859–60[12] reveals that this band was still very active, and also supplies a valuable clue as to the reason it was not found on the Dorsey list of 1880. The Hayden information states that the True Minneconjou chief was "The Elk That Whistles Running,"[13] another name for the powerful old Minneconjou chief, Lame Deer. One of the most hostile of the Sioux leaders during the Sioux War of 1876, Lame Deer's band took part in all the fighting against the United States troops during that conflict. A report from the hostile Sioux camp near Slim Buttes in September, 1876, identifies portions of the Minneconjous in the camp as those of the "Grandmother" band led by Fast Bull.[14] The use of the term "grandmother" is difficult to explain, unless it referred to the parent group of the Minneconjous. There would be little basis for a contention that this "Grandmother" band and the True Minneconjous were one and the same were it not known that the "Grandmother's" chief, Fast Bull, was Lame Deer's son. Actually Lame Deer was still the bands' chief, but during the hostilities, the war leaders, or head soldiers such as Fast Bull, were afforded more notoriety than the older council chiefs.

During the winter of 1876–77 the Sioux were hard pressed by the Army, and nearly all of the hostiles surrendered or fled to Canada with Sitting Bull. Lame Deer's band was the only group that attempted to continue to roam freely in the Powder river country of Montana. On May 7, 1877, troops under Colonel Nelson A. Miles attacked and destroyed the True Minneconjou camp on the upper reaches of the Rosebud. Lame Deer was among those who were killed.[15] Survivors of the camp, led by Fast Bull, straggled into Red Cloud Agency in September and surrendered. During the following year many of the surrendered hostiles became dissatisfied with their existence at the Sioux agencies and sought refuge in Canada. Large numbers fled from the Red

[8] Lewis and Clark gave the Minneconjous 100 lodges, 250 warriors, and 750 people, and the combined Saone groups, 120 lodges, 300 warriors, and 900 inhabitants.

[9] DORSEY, J. OWEN. *Siouan sociology*, 15th Ann. Rep. Bur. Amer. Ethnol.: 220. 1897.

[10] CURTIS, E. S. *The North American Indian* **3**: 168. New York, 1907–1930.

[11] CULBERTSON, THADDEUS, *Journal of an expedition to the Mauvaises Terres and the Upper Missouri in 1850*, Bur. Amer. Ethnol. Bull. 147: 135, 136. 1952.

[12] HAYDEN, F. V. *Contributions to the ethnography and philology of the Indian tribes of the Missouri Valley:* 375, 376. Philadelphia, 1862.

[13] Other places it is written "The Elk That Bellows Walking," but the meaning intended is probably Noisy Walking Elk.

[14] Lt. Col. George P. Buell to the Assistant Adjutant General, General Terry's Column in the field, September 9, 1876, Record Copy of Letters Sent, Post at Cheyenne River Agency, Records of the War Department, National Archives.

[15] Report of the Secretary of War for 1877: 498.

Cloud and Spotted Tail groups during the relocation of those agencies on the Missouri during the late fall of 1877.[16] Numerous small groups left at other times. The definitely hostile attitude of the True Minneconjous would make it almost a certainty that the survivors of that band were among those who joined Sitting Bull north of the border. When the Sioux returned from their Canadian exile in 1881 and were sent back to their respective agencies a year later, the True Minneconjous had ceased to exist as a distinct band. Apparently lacking the strong leadership of the Lame Deer type, the remainder of the camp had either become absorbed by the other Minneconjou bands, or had followed relatives and friends into the camps of the Oglalas and Hunkpapas while in Canada.

Examination of the material available dealing with the second band on the Lewis and Clark list, the Wan-nee-wack-a-ta-o-nelar (Tabeau lists it third, as Waniwacteonilla) reveals that by 1880 it was no longer a Minneconjou band, and as a group was nearly extinct because of its wild and hostile activities. Although the name is difficult to translate because of the differences in the Sioux words of 1800 and those of more recent vintage,[17] this band is the one mentioned intermittently throughout Sioux history as the Broken Arrows. It had its origins among the Minneconjous, later became part of the Brules, and finally is last mentioned as living with the Oglalas on the Pine Ridge reservation.

The Broken Arrows remained with the Minneconjous until at least the mid 1830's. The Atkinson-O'Fallon expedition met the camp near the Missouri in 1825. One member of the party recorded in his journal that this band was "not well looked upon by the other bands of Sioux, being considered rather refractory and ungovernable."[18] On the treaty negotiated by the Atkinson-O'Fallon party with the Saone group, which included the Minneconjous, one of the signers was a leading warrior of the Broken Arrow camp, *Chante Wahneecha*, or No Heart. This was the name given to the chief of the Broken Arrows by Lewis and Clark, but since the warriors leaders were invariably younger men, Atkinson and O'Fallon's No Heart was probably the son of the Lewis and Clark chief. Several years later, George Catlin was on the Upper Missouri and painted a number of portraits of prominent Sioux around Fort Pierre, including two Broken Arrow leaders, *Shonka*, the Dog, and No Heart.[19]

It was not long after Catlin's visit that Broken Arrows appear to have migrated, perhaps forcibly, from the Missouri to join the Brules living between the White river and the Platte. Actually the evidence points to a split in the band, with the majority joining the Brules and only a small number staying with the Minneconjous. The No Heart family, definitely identified as Broken Arrows, remained on the Missouri and were known as leaders of the Minneconjous up through the reservation period.[20] Culbertson's band list of 1850 does not refer to the Broken Arrows among the Minneconjous,

[16] HYDE, *op. cit.*: 299–301.

[17] The Dorsey material (1884) calls the band *Wan-nawega*, Broken Arrows. This is phonetically very different from the band's early identity. See also footnote 22.

[18] *Journal of the Atkindon-O'Fallon Expedition*, edited by Russell Reid and Clell Gannon, North Dakota Hist. Quart. **4**(1): 21. Oct. 1929.

[19] CATLIN, GEORGE. *Illustrations on the manners, customs, and conditions of the North American Indian*, **1:** 223; **2:** 190, 192. Catlin's description of *Shonka* fits into the general pattern of remarks we have concerning the whole Broken Arrow band: "an ill-natured and surley man—despised by the chiefs of every other band . . ." While Catlin was at Fort Pierre, *Shonka* shot and killed a noted warrior of the Hunkpapa tribe, Little Bear, and had to flee to the Black Hills to escape the vengeance of the dead man's relatives. Although Catlin distinguishes between Shonka's band, the Caz-a-zhee-ta (Bad Arrow Points), and No Heart's Wah-nee-watch-to-nee-nah (Broken Arrows), we know from later evidence that both men were leaders in the same camp. This is only another instance of the artist's careless distortion of the Teton band names, a situation which makes his material difficult to work with.

[20] The Black Hills treaty of 1876 was signed for the Minneconjous by a No Heart and an "Old Man No Heart." The latter could have been Catlin's chief, for his son was born in 1844. The younger No Heart was often referred to as Little No Heart to distinguish him from his father. Little in this case alluded to age rather than size. High Hawk, who was a Brule, recorded in his winter count that in 1838 there was a fight among the members of the Broken Arrow band. This may have been the date of the separation between the *Shonka* and No Heart portions of the camp, or perhaps refers only to an incident among *Shonka's* people after they had joined the Brules.

but does list a "Bad Arms" band among the Brules. Rufas B. Sage located the Broken Arrows on the Platte prior to the date of Culbertson's information,[21] and the name "Bad Arms" (referring to weapons-arrows, rather than body extremities) cannot be associated with any group belonging to the Brules proper. Hayden's list of 1859, in some ways more complete than Culberton's, alters the Brule's Broken Arrows to the "Band with Poor Guns or Bows." This source also places a group among the Minneconjous translated very vaguely as "The Band That Kills No People." At first glance this does not appear to be the old No Heart portion of the Broken Arrows, but examination of the Sioux word, *Waktonila*, from which Hayden gets the name reveals that it is an incomplete form of the name recorded by Lewis and Clark and Tabeau, Wan-nee-wack-a-ta-o-ne-lar or Waniwacteonilla.[22]

After the date of Hayden's information there is little mention made of the Broken Arrows.[23] They were known to have been in the hostile camp in the Powder river country in 1866 which supplied the warriors for the siege of Fort Phil Kearney, the Fetterman fight, and the other actions comprising the Red Cloud War.[24] When J. Owen Dorsey made his inquiries for his study of the Teton bands, none of his informants mentioned the Broken Arrows as being a Brule group. In fact, even his early information on the Minneconjou groups omitted the Broken Arrows. However, Dorsey made subsequent inquiries among the Minneconjous and was told by a member of the No Heart family that the Broken Arrows belonged in the Minneconjou tribal circle, but at that time (1884) they were nearly extinct.[25] From this it appears that there was a camp of Broken Arrows still in existence, but it was not living with the other Minneconjou bands attached to the Cheyenne River Agency. No Heart knew of the camp while the other Minneconjou informants did not, probably because of the old ties his family had with it.

Fortunately, Phillip Wells, a long-time Army scout, interpreter, and Indian service employee, recorded before his death, a number of his experiences among the Sioux.[26] One of these supplies us with enough information to locate the remnants of the Broken Arrow camp. In 1892, when Wells was an agency farmer on the Pine Ridge reservation, the so-called Two Sticks Raid took place which culminated in the killing of several cowboys in the Pine Ridge beef camp. Wells wrote that the Two Sticks camp was the last of the old Broken Arrow band. He said the Broken Arrows had always been a renegade group, containing many a Sioux who had been disgraced in his own band and was invited by the Broken Arrows to join them. They ignored tribal law, custom, and religious beliefs.

According to the Wells manuscript, the people in the Two Sticks camp claimed the earliest Broken Arrow chief which they could remember was *Shonka*, the Dog. This was the headman whose portrait Catlin painted at Fort Pierre in 1832. It was shortly after Catlin's visit that the Broken Arrows split up, with one portion leaving the Missouri. The Two Sticks camp was the remains of this group. Their memory of *Shonka* as their chief strongly suggests that it was his following in the band that left, or was driven out by the Minneconjou

[21] SAGE, RUFAS B. *Wild scenes in Kansas and Nebraska:* 68. Philadelphia, 1855.

[22] Catlin's phonetic vocabulary (1832) has arrow as *wonhee*. The exact translation of *waktonila* is not known, but at one time it apparently referred to something broken. By coincidence there is a more modern Teton word sounding somewhat similar; *wa-cin-ko-ke-la*, meaning easily made angry. From what we know of the Broken Arrows this could be one general description of them.

[23] During the early 1850's the Oglala and Brule Sioux on the Platte were involved in several brushes with troops stationed at Fort Laramie. These events, which led to Gen. W. S. Harney's campaign against the Sioux in 1855 and 1856, included the firing upon a military skiff crossing the Platte in 1853 and the so-called Grattan "massacre" a year later. In both instances, official reports state that a wild band of Minneconjou troublemakers, otherwise unidentified, were in some way responsible for the hostilities. There is every reason to assume that these troublemakers were the Broken Arrow camp.

[24] See the Col. Henry B. Carrington papers in Senate Document no. 32, 50th Congress, 1st Session, p. 29.

[25] DORSEY, *op. cit.*: 220.

[26] The Wells material is owned by his daughter, Miss Flora Wells, of Pine Ridge, S. Dak., who recently lent it to George E. Hyde. My information on the Broken Arrow references comes from correspondence with Hyde. Some Wells accounts were published in 1948 in North Dakota Hist. **15**(2–4), but these omitted some of the more important Broken Arrow material.

leaders, and migrated to the White river country of the Brules. No Heart, the other Broken Arrow leader identified by Catlin, remained near the Missouri, and he and his small following were absorbed by the other Minneconjou camps.

The remaining band on the Minneconjou lists, the Tar-co-eh-parh or Tacohiropapais is not mentioned in any of the available sources written after the date of the Lewis and Clark report. They either ceased to exist as a distinct band, or were known down through later chapters of Sioux history under another name. While the scattered and inconclusive evidence affords no concrete proof for such a contention, it is believed that the Tacoropas did not dissolve as a tribal band, but became more familiarly known as the *Oohenompa* or Two Kettle Tetons.

The earliest mention of the Tacoropas was recorded by Jean Baptiste Truteau in a portion of his journal kept while residing in the Arikara village during the summer of 1795. In June of that year two men from the Tacoropa camp brought a warning that three "villages" of Sioux were assembling a force of 500 warriors for an attack upon the Arikara village. Later on, in July of the same summer, the entire Tacoropa camp, said by Truteau to number "80 huts", came on a trading trip to the Arikaras.[27] Truteau's evidence shows that these Tacoropas were friendly with the Arikaras at a time when very few of the other Teton bands were on good terms with that tribe. This fact is important in attempting to connect them with the later-day Two Kettles, for it is known that the Two Kettles were the friendliest of the Teton tribes, often managing to maintain amicable relations with other tribes, and the whites as well, with whom the Tetons were then at war. In addition, the Two Kettles also undertook several attempts at farming on their own prior to the reservation days. Their agricultural knowledge and training could easily have come from their association with the Arikaras, who depended upon the raising of crops for a good part of their food supply.

Truteau's statement that the Tacoropa village numbered 80 lodges seems to be an error, for ten years later, Lewis and Clark gave the entire Minneconjou tribe only 100 lodges. Whether the latter figure includes the Tacoropa camp is not clear. The explorers failed to record the name of this band's chief (they had such information for every other Sioux band), and this gives rise to a suspicion that their information regarding these people was very vague. The figures of Truteau and Lewis and Clark can perhaps be reconciled by the suggestion that it was the entire Minneconjou tribe, and not just the Tacoropas that visited the Arikara village on July 20, 1795. The name Minneconjou, meaning "those who plant by the water" indicates that this tribe was at one time agriculturally inclined. Some tribal traditions say that the water near which they planted was the Missouri, perhaps under the guidance of the Arikara. But, why then didn't Truteau write Minneconjous instead of Tacoropas?

In the midst of all these suppositions and conjectures regarding the Minneconjous, Tacoropas, and Two Kettles, one known fact stands out which clearly links them together. Four Bears, a Two Kettle leader of the reservation period, related to E. S. Curtis, when the latter was collecting material for his work on the Sioux, a tribal tradition which Curtis used as an explanation for one of the entrys in the High Hawk winter count.[28] High Hawk, and the Baptiste Good winter counts as well,[29] recorded that in 1791 "The White Men Came and Carried the Flag Around the Nation". The Four Bears account related that when the Sioux were living "in the eastern forest", a party of whites came to a large camp of Minneconjous, Two Kettles and Sans Arcs, and pursuaded four of the chiefs to return with them to the white settlements.

Four Bears was born in 1834, and his grandfather, Two Lance, was among the chiefs who were selected to make the trip. Thunder Ring, listed by Lewis and Clark as chief of the True Minneconjou band, also went with the white party. The fact that this event took place in the spring ("the time of greening grass"), plus the reference to the "eastern forest" indicates that the Minneconjou-Two Kettle-Sans Arc camp was at-

[27] TRUTEAU, *op. cit.*: 454, 473.

[28] CURTIS, *op. cit.*: 168.

[29] 10th Ann. Rep. Bur. Amer. Ethnol.: 310. 1893.

tending one of the annual trading fairs held on the upper reaches of the St. Peter or Minnesota river.[30] In addition to the various groups of Sioux, the Tetons from the Missouri, the Minnesota bands, and the Yanktons from the Des Moines River, these fairs were regularly attended by British fur traders from Canada. The traders, in taking the chiefs to their posts and presenting them with medals and other gifts, were trying to retain the allegiance of the western Indians after the close of the Revolutionary War. The trip was evidently a long one, for the Teton chiefs did not return to their people until the following winter.

Combining the evidence contained in the Four Bears account with the known conditions of the time regarding the Sioux trading fairs and the activities of the British among the western Indians, it certainly appears that the tradition could be dated anytime between 1785 and 1795. The year of the winter count entry, 1791, is as good a selection as any.

The important fact to be noted here is that Two Kettle tradition, handed down to the time of Four Bears, discloses that in 1791 elements of that band roamed together with the future Minneconjou and Sans Arcs groups in one large Saone camp. By 1803 the Minneconjous had emerged into a distinct tribe, containing three subbands, while the Sans Arcs as well were a recognizable group on the Tabeau and Lewis and Clark lists. It does not seem too unreasonable to suggest that the Two Kettle portion of the old Saone camp was still with the Minneconjous as the mysterious sub-band of that tribe called the Tacoropas. We are able to follow the progress of the other two Minneconjou bands, the true Minneconjous and the Broken Arrows, through subsequent pages of Sioux history, but after 1803 the Tacoropas disappeared, and almost corresponding with their disappearance came the advent of this new group, the Two Kettles.

THE SAONES

Tabeau	*Lewis and Clark*
Tatchindi-chidja	Sah-one
Hitasiptchone	Tack-chan-de-see-char
Hont-papas	Sah-o-ne-Hont-a-par-par

[30] HYDE, *op. cit.*: 21. For this period, Hyde has located the Sioux trading fairs on the headwaters of the Minnesota river.

The group easiest to identify are the Hontpapas or Sah-o-ne-Hont-a-par-par, which clearly were the later-day Hunkpapa Sioux, the wild and warlike people made famous by the leadership of Sitting Bull. Lewis and Clark reveal that their chief was Long Dog (Shark-ka-has-car—*Shunka*, dog, *hanska*, long), a name borne by one of the more hostile Hunkpapa chiefs during the 1870's.[31] At the time of the explorers' visit, the Hunkpapas are said to have roamed on both sides of the Missouri to the north of the Minneconjous located on the Cheyenne river.[32] On the west side of the Missouri this would place them in the region of the Moreau, Cannonball, and Heart rivers, which at that time was claimed by the Arikaras. It is suspected that the Hunkpapas spent most of their time east of the Missouri, crossing the river only to hunt buffalo and to trade with (and steal from) the Arikaras. It was not until some 20 years later, when the power of the Arikaras was broken by a combination of disease, Sioux hostility, and their own stupidity, that the Hunkpapas could move across the Missouri and lay any sort of substantial claim to the lands west of the river.

Tabeau's second Saone band, Hitasiptchone (Itazipcho to modern ethnologists), is far better known by its French translation, Sans Arcs. It is a combination of the Sioux words *itazipa*, meaning bow and *cho*, an abbreviation of *chodan*, or without. For some unknown reason this name did not appear on the Lewis and Clark list. Perhaps it was not commonly used and the band was known by another name. After examining Tabeau's first band, Tatchindi-chidja, and Lewis and Clark's Tack-chan-se-see-char, it would seem that these names also refer to the group eventually to become known as the Sans Arcs. Lewis and Clark received much of their original information regarding the Sioux from Tabeau, and were able to check and rearrange it on the basis of material obtained from Hugh Heney, a Sioux trader, and from other informants at the Mandan village.[33] It is very possible that Heney told them that the Sans Arcs were better known as the Tack-chan-de-see-char band, thus account-

[31] Report of the Secretary of War for 1876: 481, 483.

[32] THWAITES, *op. cit.*: 97, 98.

[33] TABEAU, *op. cit.*: 101, 102.

ing for the omission of Tabeau's Hitasiptchone from their list.

While the meaning of the name Tatchindi-chidje (Tack-chan-de-see-char) is not perfectly clear, it is fairly obvious that it refers in some manner to the weapon, the bow. This, however, is not the only reason for believing that this band was the fore-runner of the later-day Sans Arcs. Again the name of the band's chief as supplied by Lewis and Clark aids in its identification. They write it as War-mun-de-o-pe-in-do-tar, the best translation of which may be Red Tailed War Eagle. There was a leading chief of the Sans Arcs by this name during the 1850's and '60's. He signed the Sans Arcs treaty of 1865 and his name was then written almost exactly the same phonetically as was Lewis and Clark's chief.[34]

There are three possible translations of the band name, Tack-chan-de-see-char or Tatchindi-chidja. The first, and not necessarily the best, would be "bows from the heartwood of the willow": tack (tat) from *iTAZipa* or bow, chan-de (chandi) from *chante* or heart, and see-char (chidja) from the ending of *choh-wan-zhi-cha* meaning willow. A second choice, somewhat similar to the first, can be obtained by substituting the word *schicha* meaning bad, for see-char or chidja. This would result in something like "bad bows from the heartwood". The third possibility would be "a bow, the back of which is overlaid with sinew". Here the work *takan*, meaning the sinews taken from the backs of deer and buffalo and used extensively in making bows, would be combined with *itazipa* (bow) to form Ta-kan-i-ta-zi-pa.

Regardless of what the true translation may be, one thing is evident from the inclusion of this name on both the Saone band lists; that the name Itazipcho (without bows) signifying the Sans Arc Sioux had its origin not too many years before the first Teton contact with the whites on the Missouri. It certainly appears that Tack-chan-de-see-char (Tatchindi-chidja), whatever its exact meaning, was the earliest of the names applied to what we know today as the Sans Arcs. Of the five tribes making up the Saone faction, the Sans Arcs grew to be the third largest (after the Minneconjous and Hunkpapas) and were certainly living among the Tetons on the Missouri at the time of the visits by Tabeau and Lewis and Clark. Unless perhaps they were recognized by the sub-heading Sah-one by Lewis and Clark, the Tack-chan-de-see-char group can be the only name on their list which refers to the Sans Arcs. As has been mentioned, perhaps the Hitasiptchone included on Tabeau's list along with the Tatchindi-chidja was another, newer name for the band and was imitted from the Lewis and Clark list upon consultation with Heney or someone else. It also may be that the Hitasiptchone was an offshoot of the parent Tatchindichidja group, for already the Tetons or Bois Brules had four subbands and the Minneconjous three. The fact that this offshoot grew and prospered while the parent group, the Tatchindi-chidja, disappeared from any later references is not unusual. Examination of the Minneconjou band lists obtained in the 1880's fails to disclose the parent group of that tribe, the True Minneconjous, which was listed by both Tabeau and Lewis and Clark.

There remains then only one name on the two lists which thus far has not been identified, the Sah-one sub-group of Lewis and Clark. It is impossible to even guess with any degree of accuracy what this name made reference to. There are two Teton tribes, the Two Kettles and the Blackfeet Sioux, that are not mentioned in any recognizable way by Lewis and Clark, and it may be that one of these is the Sah-one band. It is felt, however, that the Two Kettles were more closely associated with the Minneconjous and possibly, at that date, were the mysterious Tacoropa band. It also does not seem likely that the Blackfeet were the group referred to by Lewis and Clark, for Tabeau provides a clue which strongly suggests that the Blackfeet were then part of the Yankton tribe. On his list, under the heading of "The Yinctons of the South" living on the James river, is a band called Seascapé, a name almost identical to Sihasapa or Blackfoot (*siha*, foot and *sapa*, black). It is known that the Blackfeet were the last of the Teton tribes to cross the

[34] The first signer for the Sans Arcs at Fort Sully on October 20, 1865, was Wah-mun-dee-o-pee-doo-tah, or The War Eagle with the Red Tail. *Wahmundee* or *Wambili*, eagle or war eagle, and *duta* or *luta*, red. Actually *luta* means scarlet, but it was commonly translated as red.

Missouri, for even as late as 1825, the Atkinson-O'Fallon treaty commission met them on the east bank of the river, while the other Teton groups, including the Hunkpapas, were all treated with on the west bank.[35] The delayed arrival of the Blackfeet in the regions west of the Missouri could very well have been caused by the time-consuming break with their old ties among the Yanktons and a gradual movement northward up the Missouri to where they contacted the Hunkpapas and crossed westward.

[35] The Commission's report states that the Saones were divided into two groups which roamed on both sides of the Missouri. One group, the Minneconjous, Sans Arcs, and Two Kettles, signed a treaty at the mouth of the Teton (Bad) river on July 12, 1825, and were the Saones who generally inhabited the country west of the Missouri. On July 12, the Commission concluded a treaty with the Saones of "Fire Heart's band" at Camp Hidden creek. This was on the east bank. Fire Heart's band were the Blackfeet-Sioux. The Fire Heart family were long-time leaders of that tribe.

NEW BOOK ON AQUARIUM FISHES YIELDS DISTRIBUTION DATA

There are 40,000 kinds of fishes—species and subspecies—in the world. This is the most recent estimate of Dr. Leonard P. Schultz, Smithsonian Institution curator of fishes. The fishes can be divided roughly into inhabitants of eight world zones, by far the richest of which is the tropical Indo-Pacific area. This is the region extending from the head of the Red Sea to Easter Island. Within this region are found approximately 9,000 species.

The number of kinds in other regions, as estimated by Dr. Schultz, is as follows: Australia, 1,500; North America, 4,500; South America, 6,500; Africa, 6,500; deep seas all over the world which have their own characteristic forms of fish life, 2,500; Europe, Asia, and India, 6,500; various island groups, 3,000.

These estimates are contained in a Handbook of Tropical Aquarium Fishes suitable for the home aquarium written by Dr. Schultz in cooperation with Herbert R. Axelrod. However, among all these fishes fewer than 500 kinds are common in aquaria.

All over the world, Dr. Schultz points out, fishes have evolved into a remarkably diverse group to fit various habitats and to follow various ways of life. Some fly, but not like birds; others leap, walk, and burrow as well as swim. Some swim as fast as a locomotive—60 miles an hour for the swordfish, 50 for the bonito, 44 for the tuna. Some, like the bass, hardly can speed up beyond about 12 miles an hour.

Some of their varied ways of life are shown by the bettas and the paradisefish, both of which blow bubble nests at the water surface where they cradle their eggs and babies. Others, like the male of the black-chinned mouthbreeder, incubate the eggs and young in the mouth. More remarkable is the seahorse, the male of which has a pouch, much like that of a kangaroo, where the eggs are placed by the female and where they are incubated for a few weeks before birth occurs.

Dr. Schultz has carried on special studies of fish flight on South Pacific marine flyingfishes. "Anatomically," he says, "flyingfishes do not possess muscles that could possibly help the fins to flap like the wings of birds. Instead the fins are used as glider wings. Although some of the oceanic flyingfishes can sail for over 30 seconds at about 35 miles an hour, the freshwater flyingfishes of South America can sail only a few yards. These little aquarium fishes are thin as a wafer and shaped like a hatchet."

Walking, Dr. Schultz points out, is well developed among some fishes. The lower rays of one of the fins are separated from one another and are controlled by special muscles so that they propel the fish along the bottom in a fashion very similar to the walking of a mammal on land. The fish that can "walk" best on land is a common aquarium fish, the walking or climbing perch, but it does not have the separate fin rays. It does have an extra air chamber above the gills that aids it in staying alive for several hours out of water. This walking perch has been known to travel at least 300 feet over dry land, in a space of 30 minutes in going from one pool to another.

Nearly all fishes, Dr. Schultz says, are well equipped with special sense organs to acquaint them with their immediate environment. Connected with a highly developed sense of touch is the so-called "lateral line," which enables a fish to detect low frequency vibrations in the water, such as would be set up by the movements of another fish. In sharks, for example, this is so highly developed that they can tell whether the movements are made by a healthy or an injured fish.

ENTOMOLOGY.—*Studies in Panama* Culicoides (*Diptera: Heleidae*), *VI: The* hylas *group of the subgenus* Hoffmania.

WILLIS W. WIRTH[1] and FRANKLIN S. BLANTON.[2]

Among the Panama biting midges of the genus *Culicoides* is a group of species related to *hylas* Macfie whose identity and synonymy until now have been in serious confusion. In this group are the species described in part as *rozeboomi* by Barbosa (1947) and reported under that name in *Culcoides* biology and control studies by Carpenter (1951) and Woke (1954) from determinations furnished by the senior author. The collection of a large number of both sexes of these species by the junior author in Panama by means of light traps and horse-baited traps made possible a taxonomic analysis of the populations which resulted in a revision of species concepts and brought out points of synonymy as discussed below.

The methods and terminology used are explained briefly in Part I of this series (1953). We use the Tillyard modification of the Comstock-Needham system of wing venation; wing length is measured from the basal arculus. The antennal ratio is the value obtained by dividing the combined lengths of the last five antennal segments by the combined lengths of the preceding eight. Measurements are of single specimens unless they are followed by values in parentheses, in which case the values are "mean (minimum–maximum, n = number of measurements)".

We are greatly indebted to Irving Fox, of the University of Puerto Rico at San Juan, for his kindness in lending us the type of *heliconiae* Fox and Hoffman, and to Paul Freeman, of the British Museum (Natural History), for his valuable assistance in comparing our specimens with the types of *hylas* Macfie, *palpalis* Macfie and *verecundus* Macfie, and for notes which aided us in the recognition of these species.

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KEY TO THE SPECIES OF THE HYLAS GROUP OF CULICOIDES

1. Second radial cell included in a pale spot at its apex; base of cell M_4 dark where it borders the bases of veins M_{3+4} and Cu_1 and a pale spot present anterior to base of vein M_1; apices of veins M_1 and M_2 and usually of veins M_{3+4} and Cu_1 pale (*hylas* group of subgenus *Hoffmania*) 2

   Second radial cell dark (various subgenera); or base of cell M_4 pale where it borders veins M_{3+4} and Cu_1 and no pale spot anterior to base of vein M_1 (other groups of subgenus *Hoffmania*); or apices of veins M_1, M_2, M_{3+4} and Cu_1 dark (subgenus *Culicoides*)

2. Hind femur dark to apex; apices of veins M_{3+4} and Cu_1 usually pale 3

   Hind femur with subapical yellow band; apices of veins M_{3+4} and Cu_1 dark; midknee broadly yellow on each side; mesonotum yellow in yellow in middle with dark-brown sublateral vittae; third palpal segment slender and without pit; wing 1.3 mm long *verecundus* Macfie

3. Midknee yellow at the joint; mesonotum dark gray in middle, nearly unicolorous blackish; clypeus much elongated, bases of palpi arising far below ventral eye margin; third palpal segment slender, without pit; wing 1.5 mm long ..... *heliconiae* Fox and Hoffman

   Midknee black with narrow pale rings on each side; mesonotum yellow in middle with brownish-black sublateral vittae; clypeus not elongated, bases of palpi arising at level of ventral eye margin; third palpal segment swollen, with a shallow, subdivided pit; wing 1.2 mm long ..... *hylas* Macfie

Culicoides heliconiae Fox and Hoffman

Fig. 1

Culicoides sp. Fox, 1942, Puerto Rico Journ. Publ. Health and Trop. Med. **17:** 418 (pupa; Venezuela; fig. respiratory horn, anal segment).

Culicoides heliconiae Fox and Hoffman, 1944, Puerto Rico Journ. Publ. Health and Trop. Med. **20:** 108 (male, female; Maracay, Venezuela; reared ex bromeliads; fig. wing); Fox, 1948, Proc. Biol. Soc. Wash. **61:** 22 (male, female redescr.; fig. palpus, male aedeagus, parameres; rec. Trinidad [in stable], Honduras [pupae from water-holding plant]); Ortiz, 1950, Rev. San. Asist. Social **15:** 450 (discussion).

Culicoides rozeboomi Barbosa, 1947, An. Soc. Biol. Pernambuco **7:** 26 (male, female; Trinidad, Peru, Panama; fig. palp, mesonotum, wing, male genitalia). NEW SYNONYMY.

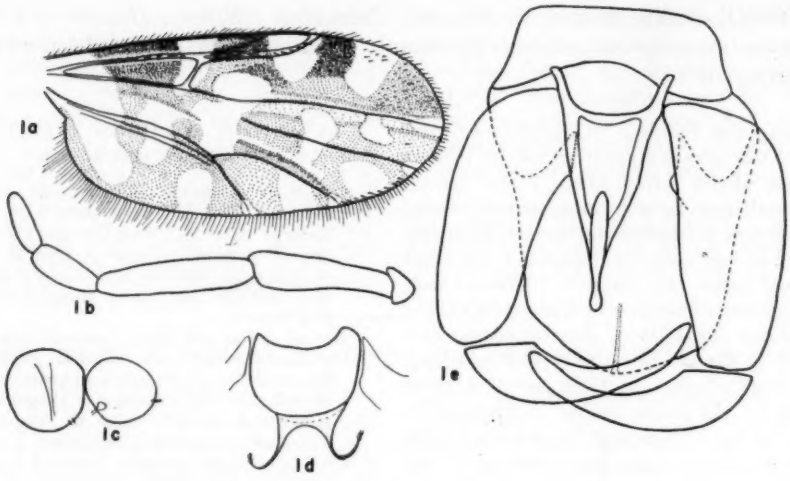

FIG. 1 HELICONIAE

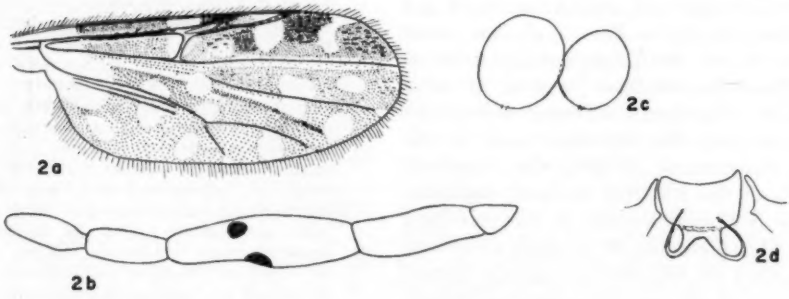

FIG. 2 HYLAS

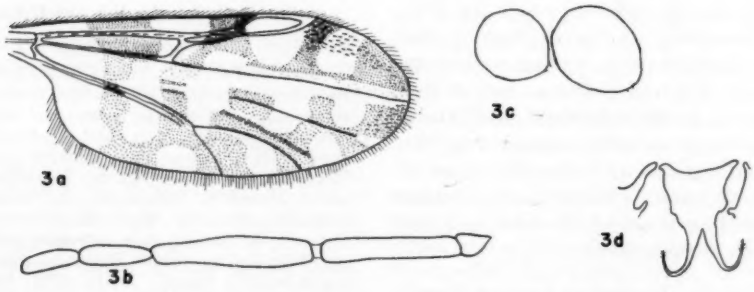

FIG. 3 VERECUNDUS

FIGS. 1–3.—*Culicoides heliconiae* Fox and Hoffman; 2, *Culicoides hylas* Macfie; 3, *Culicoides verecundus* Macfie. (*a*, female wing; *b*, female palpus; *c*, female spermathecae; *d*, male parameres; *e*, male genitalia, parameres removed.)

Culicoides verecundus Ortiz and Mirsa (misident., not Macfie), 1952, Rev. San. Asist. Social **17:** 259 (male, female redescr.; Miranda, Venezuela; fig. female antenna, spermathecae, wing, male palpus, genitalia) (*verecundus* Macfie = [*palpalis* Macfie], ? = *heliconiae* Fox and Hoffman).

Barbosa (l.c.) described *rozeboomi* from a series of 1 male and 19 females from various localities without designating a holotype. We have examined the type series in the U. S. National Museum and find that it is composed of several species. We have selected as lectotype the specimen first listed by Barbosa, a pinned female from Trinidad collected by Rozeboom, of which Barbosa figured the palpus. A wing, palpus, antenna, and hind leg are mounted separately on a slide. This specimen agrees in all respects with *heliconiae*, of which *rozeboomi* becomes a synonym.

Through the courtesy of Dr. Irving Fox, of the University of Puerto Rico, we have studied the slide-mounted holotype of *heliconiae* and find that although it is in only fair condition the characters of the head, legs, and abdomen agree very well with our material of *heliconiae*, which we characterize briefly below. Fox and Hoffman's figure of the wing, from the type slide of *heliconiae*, is entirely misleading. It is apparent that the type is a teneral specimen and the wing had become somewhat dirty and damaged before being mounted.

Female.—Wing 1.46 (1.22–1.68, n = 26) mm long. Eyes bare, contiguous for a distance of only one corneal facet. Antenna with flagellar segments in proportion of 22:20:20:20:20:20:20:20: 25:26:32:34:48, antennal ratio 1.04–1.09, distal sensory tufts present on segments III, XI–XV. Clypeus much elongated, the basis of the palpi arising far from level of ventral eye margin. Palpal segments (Fig. 1*b*) in proportion of 6:21:30:15:12, third segment slender, 3.3 (2.8–5.7, n = 18) times as long as broad, without sensory pit. Mandible with 23 (18–26, n = 26) teeth. Thorax blackish, mesonotum with median area and other small areas dark pruinose gray, lateral margins and a sublateral pair of short bands velvety black; humeri whitish. Legs black; mid knee narrowly pale on each side of joint; fore femur with subapical, fore tibia with subbasal and hind tibia with basal and apical, narrow pale rings; comb of hind tibia with six spines, the second from the spur longest. Wing (Fig. 1*a*) with costa extending to 0.68 of wing length, macrotrichia sparse in apices of cells R_5, M_1 and M_2; with pattern as figured, apices of veins M_{3+4} and Cu_1 pale (dark in Cerro Campana series and in some El Valle specimens); the pale spot in cell M_4 continued proximad along vein M_{3+4} to base of mediocubital fork. Halter yellowish white. Abdomen black, cerci yellowish; spermathecae (fig. 1*c*) two, unequal, subspherical, measuring 0.065 by 0.053 and 0.051 by 0.042 mm, respectively.

Male genitalia (Fig. 1*d*, *e*).—Ninth sternum with scarcely perceptible caudomesal excavation; ninth tergum rounded caudad without apicolateral processes, the caudal margin with a distinct mesal, internal, longitudinal sclerotization. Basistyle without mesal spines, dorsal and ventral roots not developed; dististyle curved, with slender apex, yellow in color, contrasting with the dark brown basistyle and tergum. Aedeagus twice as long as basal width, a heavily sclerotized, transverse band at basal sixth; apex with internal peg and slender, ball-like tip. Parameres fused on basal two-thirds, the fused part resembling a truncated cone in outline and slightly broader than long, the bases of the free portions widely separated, the free portions extremely slender, almost filiform and minutely pubescent at their tips.

Material examined.—As follows:

PANAMA: Canal Zone—a long series of females from Fort Sherman, Fort Kobbe (horse traps), Loma Boracho, and Madden Dam. Chiriquí Prov. David, 18 December 1952, 2 females; Tortugas, 16 December 1952, 2 females; El Volcán, December 1952, 1 female. Coclé Prov. El Valle, 25 June 1954, 25 Med. Det., 3 females; Penonome, 23 November 1952, 1 female. Panamá Prov. Cerro Campana, 19 December 1952, 6 females.

PERU: Río Charape, 14 September 1911, C. H. T. Townsend, 5 females (paratypes of *rozeboomi*).

TRINIDAD: 1 female, without locality, collected by L. E. Rozeboom (lectotype of *rozeboomi*). Port of Spain, June 1953 (light trap), 5 males, 16 females. Lalaja Trace, 5 June 1955, T. H. G. Aitken, reared from flowers of *Heliconia bihai*, 2 males, 1 female, 2 pupal exuviae.

VENEZUELA: Maracay, 2 August 1927, 1 female (holotype of *heliconiae*, in Univ. Puerto Rico coll.). San Felipe, 22 May 1951, I. Ortiz, 1 male, 2 females (Ortiz redescribed other specimens from this series as *verecundus* Macfie).

Culicoides hylas Macfie
Fig. 2

Culicoides hylas Macfie, 1940, Ent. Monthly Mag. **76:** 26 (female; British Guiana; fig. wing).

Female.—Wing 1.19 (0.99–1.29, n = 24) mm long. Eyes bare, contiguous for a distance equal to the width of three corneal facets. Antenna with flagellar segments in proportion of 25:20:20:20:20:20:20:22:30:30:37:39:53, antennal ratio 1.12 (1.06–1.15, n = 15), distal sensory tufts on segments III, XI–XV. Clypeus not elongated, bases of palpi arising at level of ventral eye margin. Palpal segments (Fig. 2*b*) in proportion of 8:30:50:23:17, third segment slightly swollen 3.1 (2.6–3.6, n = 17) times as long as broad, with shallow, subdivided pit, which is often completely divided into two small, separate, round pits. Mandible with 19 (17–21, n = 26) very minute teeth. Thorax dark brown, mesonotum with median area yellow and a pair of brownish black, broad, sublateral vittae; humeri blackish. Legs brownish black; fore and mid knee spots black, with adjacent narrow yellow rings on tibiae and femora, hind femur entirely dark, hind tibia with narrow pale rings at base and apex; comb of hind tibia with six spines, the second from the spur longest. Wing (Fig. 2*a*) with costa extending to 0.68 of wing length, macrotrichia fairly numerous in apices of cells M_5, M_1 and M_2 and very sparse in cell M_1 and anal cell; pattern as figured, apices of veins M_1 and M_2, and in some specimens ends of M_{3+4} and Cu_1 pale, extent of other markings somewhat variable, the distal pale spot in cell R_5 meeting wing margin, chevron shaped, with the notch on the distal side of the spot; the pale spot in cell M_4 not continued proximad along vein M_{3+4}. Halter whitish. Abdomen blackish, cerci yellow; spermathecae (fig. 2*c*) two, slightly ovoid, unequal, measuring 0.057 by 0.044 mm and 0.044 by 0.038 mm, respectively, the bases of the ducts not sclerotized.

Male genitalia (Fig. 2*d*).—Indistinguishable from those of *heliconiae* except for the shape of the base of the parameres, which in *hylas* is much shorter and more quadrate, about twice as broad as long.

Material examined.—As follows:

HONDURAS: Lancetillo, May 1954, P. Galindo, 6 males, 9 females.

NICARAGUA: Villa Samoza, September 1953, P. Galindo, 3 females.

PANAMA: A long series of males and females collected by Blanton in light traps from: Bocas del Toro Prov.—Almirante. Canal Zone—Fort Clayton, Fort Kobbe, Fort Sherman, Gamboa, Loma Boracho, Madden Dam, Mojinga Swamp. Coclé Prov.—El Valle, Rio Hato. Colón Prov.—Piña. Darién Prov.—Garachine, Jaqué. Panamá Prov.—Arraijan, Cerro Campana, La Jolla, Pacora. Veraguas Prov.—Sapotilla, 14 July 1953. The following type material of *rozeboomi*: Balboa, C. Z., 3 July 1942, P. A. Woke, no. 1025, 1 male (allotype); 30 June 1942, P. A. Woke no. 1022, 1 female. Barro Colorado Island, C. Z., November 1941, J. Zetek, no. 4920, bred from *Calathea violacea*, 19 females (paratypes). Barro Colorado Island, November 1934, J. Zetek no. 3425, from fruits of *Ficus*, 2 females (paratypes). Cabima, 19, 22 May 1911, A. Busck, 4 females (paratypes).

PERU: Iquitos, March, April 1931, R. C. Shannon, 1 female (paratype of *rozeboomi*).

Culicoides verecundus Macfie
Fig. 3

Culicoides verecundus Macfie, 1948, Ann. Trop. Med. Parasit. **42:** 76 (male, female; Chiapas, Mexico; fig. wing).

Culicoides palpalis Macfie, 1948, Ann. Trop. Med. Parasit. **42:** 78 (female, Chiapas, Mexico).

Culicoides rozeboomi var. *contubernalis* Ortiz and Leon, 1955, Bol. Ent. Cient. Nac. Ecuat. **67:** 574 (female; Ecuador; fig. wing, antennae, palpus, spermatheca). NEW SYNONYMY.

Female.—Wing 1.31 (1.16–1.42, n = 19) mm long. Eyes bare, contiguous for a distance equal to the width of the two corneal facets. Antenna with flagellar segments in proportion of 25:20:20:20:20:20:20:22:30:31:36:38:53, antennal ratio 1.10 (1.01–1.19, n = 7), distal sensory tufts on segments III, XI–XV. Clypeus elongated, palpi arising much below level of ventral eye margins. Papal segments (Fig. 3*b*) in proportion of 10:50:57:25:20, third segment very slender 4.2 (3.2–5.2, n = 15) times as long as broad, without sensory pit. Mandible with 23 (22–27, n = 19) teeth. Thorax dark brown, mesonotum yellowish mesad, with subshining brown sublateral vittae, and blackish along anterior and lateral margins, humeri whitish. Legs dark brown; fore and hind knee spots black, their femora with broad subapical pale bands and their tibiae with broad basal pale bands; mid femur with apex and mid tibia with base, broadly yellow, sometimes a very small black spot on apex of mid femur; hind

tibia with apex narrowly pale; comb of hind tibia with six spines, the second from the spur longest. Wing with more extensive pale markings (Fig. 3*a*) than in other species of the group, but with ends of veins M_{3+4} and Cu_1 dark; pale spot in cell M_4 continued proximad along vein M_{3+4} to base of mediocubital fork; costa extending to 0.70 of wing length, macrotrichia very sparse in apices of cells R_5 and M_1. Halter whitish. Abdomen blackish, cerci yellow; spermathecae (Fig. 3*c*) two, slightly ovoid, unequal, measuring 0.053 by 0.044 mm and 0.044 by 0.037 mm, respectively, bases of ducts not sclerotized.

Male genitalia (Fig. 3*d*) as in *heliconiae*, but the parameres with fused basal portion about as broad as long and the distal free portions not so broadly separated at their bases, which are shorter than in *heliconiae*.

Material examined.—As follows:

HONDURAS: 1 male, 4 females. Lan Cetilla, February 1954, P. Galindo.

NICARAGUA: 3 females, Villa Somoza, September 1953, P. Galindo.

PANAMA: A long series of males and females from: Bocas del Toro Prov.—Almirante. Canal Zone—Mojinga Swamp. Chiriquí Prov.—David, Volcán. Panamá Prov.—Cerro Campana, Pacora. Of special interest are 34 females from Barro Colorado Island, C. Z., August 1944, J. Zetek no. 5175, from bracket fungus (reported by Barbosa (1947) as *luteovenus* Root and Hoffman).

Remarks.—According to Paul Freeman the hind legs of the type of *palpalis* Macfie are missing, but in spite of this we follow Ortiz and Mirsa in placing the species in synonymy with *verecundus* Macfie (which has page precedence) on the basis of the broadly pale knees of the midlegs, the dark apices of veins M_{3+4} and Cu_1, and the similarity of the palpi. Macfie distinguished the two by the extent of the distal pale spot in cell R_5, which is much smaller in *palpalis*, but in our series these markings are extremely variable. From Ortiz and Leon's very excellent description and figures of *rozeboomi* var. *contubernalis*, it is evident that this Ecuadorian form is identical with *verecundus*.

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NOTES AND NEWS

AUCA INDIANS DESCRIBED

The unfortunate recent slaying of five missionaries by Auca Indians of eastern Ecuador has aroused widespread concern about the name and nature of these remote South American aborigines. Dr. Matthew W. Stirling, director of the Smithsonian's Bureau of American Ethnology, is one of the Institution's scientists who has been interrogated. Stirling, who several years ago made a first-hand study of the neighboring headhunting Jivaros of Ecuador, says that although comparatively little is known concerning these Indians there are several revealing accounts of them in the literature.

The term *Auca*, says Dr. Stirling, does not refer to any particular tribe but is a Quechua term used in the Andean highlands to refer to any pagan Indian groups as contrasted with those who have been christianized. The group who did the recent killing, he says, belongs to the Zaparo tribe and lives in the area between the Curare and Napo Rivers. The Franciscans, Jesuits, and Dominicans established a number of missions among them from the sixteenth to the nineteenth centuries. Much of what is known of these Indians is based on Alfred Simson, an Englishman who spent considerable time with the Zaparos in the 1870's. Information concerning the Zaparo and the many attempts to missionize them and their neighbors is contained also in the accounts of Pablo Maroni, José Chantre y Herrera, Francisco de Figueroa, Father Pierre, and others who have painstakingly compiled the mission and travel records covering four centuries.

"White interference," Dr. Stirling believes, "has been generally disastrous to these Indians, principally because of factors that upset the normal equilibrium of their lives. Among these were the introduction of the white man's diseases,

to which the natives had little resistance, and the unhealthy effect of compelling them to wear clothes in a tropical rain-forest environment. Although the Indians were normally quite nomadic, they were often compelled to work in the fields and perform other tasks unfamiliar to them. Then, too, they resented interference with their own religion and the attempt to substitute for it ideologies completely foreign to their own background and completely incomprehensible to them. They also found objectionable the suppression of many of their own deeply rooted social habits. The white man's ban on polygamy they found especially intolerable. Because of the hazards of warfare and hunting there were typically more women than men; thus many women found themselves without a normal home and family life, and the men without the prestige of a large family."

"Inevitably," Dr. Stirling continues, "sooner or later the Indians revolted against this restriction on their liberties. In the more remote regions where there was virtually no other white population, as among the Zaparos and Jivaros, these revolts were successful and the mortality among missionaries was high.

"In their general manner of living the Zaparos behave like the other primitive tribes of the eastern Andes. They have no political or tribal organization but live in extended patrilineal groups, not in villages but in a single large community house. As a rule they do not stay more than a few years in one locality. Lacking chiefs or leaders, the most influential individuals are the medicine men who are supposed to control supernatural forces. They are believed to control weather and the movements of game and to cure or produce disease. They are both feared and respected.

"For hunting, the Zaparos use the blowgun with poison darts. They do not know how to make the poison themselves but obtain it through trade. For fighting they use lances made of Chonta palm. They frequently make raids against other Indian groups, both of their own kind and neighboring tribes. Unlike their neighbors, the Jivaros, they do not shrink heads and only occasionally collect heads as trophies.

"The Zaparos are normally friendly to strangers traveling among them if the traveler has been properly introduced so that they are not suspicious of his motives. As they are in more or less constant contact with local traders, such introductions are not too difficult to obtain.

"They are happy in disposition and laugh and talk a great deal although somewhat shy in the presence of whites until they have become well acquainted. They are very fearful of sorcery, and should one of them fall sick while a white visitor is among them, it would be virtually certain that the visitor would be blamed and his life would be in danger. They believe in transmigration, thinking that when a man dies he is reborn as a jaguar or some other animal and that a woman becomes a beautiful bird."

"These Indians," says Dr. Stirling, "are a very practical people and frequently do not have much food reserve. Thus when a mother with a young child dies, the child is usually buried alive with her. If a man becomes extremely ill or too old to carry his own load, he is choked to death by his friends so that he will not be a burden on society.

"Their ordinary dress consists of a loincloth and some crude body and face painting, with blue and red vegetable colors. Women have considerable liberty and are mistresses of their own persons. When a man wishes to take a woman as wife, he kills a deer or paca and, returning, throws it at the feet of the woman of his choice. If she takes it up and cooks it for him, he is accepted. If she ignores it, he has been refused. Women are sometimes taken by force, a method which carries more prestige, the woman making only a sham resistance. If her resistance is real, the marriage is not carried out.

"They sometimes drink fermented manioc as a group and get completely drunk, or take datura, a drug. When in this condition they are also likely to be dangerous, not only to visitors but to one another. They raise manioc and bananas but depend mostly upon hunting and fishing for their subsistence. The men do not eat the flesh of tapir, because they believe it will make them sluggish like that animal. They also raise tobacco, which, taken in liquid form, is an almost universal remedy among them. It is also supposed to sharpen the senses.

"They are expert woodsmen and hunters with a most intimate knowledge of the habits of all jungle animals and birds. The occasional killing of a few people is an oft-repeated pattern that has been going on for the past 4 centuries, and will probably continue as long as efforts are made to change their way of life. The recent outburst might have been due to a number of things, but it would have been cause enough for the Zaparos if they realized that their visitors from the sky were missionaries.

"It is reported that a group of Zaparos who were brought to Iquitos in 1875 were entirely unimpressed by steamboats and heavy machinery, but were startled almost out of their loincloths by the sight of horses and cows."

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